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TIDES AND THE RIGIDITY OF THE EARTH

By PROF. A. E. H. LOVE, F.R.S.

THE publication of a third edition of Sir G. H. Darwin's well-known semi-popular book on the Tides¹ affords an opportunity of forming an estimate of the advances that have been made in recent years in knowledge concerning all those geophysical phenomena which either are directly due to the forces that cause the tides or share with these direct effects some feature rendering them amenable to discussion by similar methods. Sir G. H. Darwin is the greatest living authority on these questions and the fact that nearly one quarter of the third edition of his book is either added or rewritten is sufficient evidence that substantial advance has been made. This advance is not confined to the theory but extends also to the methods of observation and the devising of suitable instruments, besides including a very great increase in the mass of records that are available for the comparison of theoretical results with observed facts. On the purely observational side, perhaps the greatest novelty is to be found in the beginning of actual measurements of the range of the tide in the open sea. On the purely theoretical side, the modifications of Laplace's nebular hypothesis which have been suggested by various writers are specially attractive. But the advances that will prove most interesting to many readers have been made by combining theoretical considerations with observational results in regard to several groups of phenomena from which conclusions have been drawn as to the internal constitution of the Earth. We propose to consider these matters in order.

¹ *The Tides and Kindred Phenomena in the Solar System*, by Sir George Howard Darwin. Third Edition. London: John Murray, 1911.

TIDAL OSCILLATIONS IN THE OCEAN

By the range of the tide at a place is meant the excess of the depth of water available at high-water for floating a ship at the place above the corresponding depth at low-water. Until recently measurements of the range of the tide were made only at places close to the coasts. They are, in fact, among the results furnished by the use of a tide-gauge. It was supposed, chiefly on theoretical grounds, that the range of the tide in the open sea was much smaller. For a complete understanding of the tides, it is desirable to ascertain the range in the open ocean and in partially enclosed seas, such as the English Channel, by direct observation. It appears that ordinary methods of sounding are not available for this purpose and new instruments have been specially devised, one by Captain Adolf Mensing of the Imperial German Navy, the other by Admirals Mostyn Field and Purey-Cust. The preliminary results obtained by the use of the instrument due to the latter are very striking, a range of tide in the Channel amounting to no less than 24 ft. having been measured at a place about midway between Beachy Head and Dieppe. Systematic observations of this kind may be expected to throw much light on the nature of tidal oscillations. The extent to which the tide wave in the Atlantic Ocean, for instance, is an oscillation generated in that ocean by the direct action of the Sun and Moon, as contrasted with a progressive wave, generated in the Pacific and Southern Oceans and entering the Atlantic between the promontories of South Africa and South America, is in some degree a matter of controversy. The systematic study of the tides in the open ocean, as distinct from the ebb and flow along the coasts, may be expected to go far towards settling the question; the whole value of the method of cotidal lines, as developed by Airy and Whewell, depends upon the answer that may be obtained.

Like other natural motions, the tidal oscillations of the ocean, maintained by the attraction of the Sun and Moon, do not take place without friction; and one effect which such friction can bring about is a steady diminution in the speed of the Earth's rotation. The friction of the tides which were in the past raised by the Earth in the Moon, if the Moon once possessed oceans, may in like manner have operated to diminish the speed of the Moon's rotation; this may be the reason why the Moon now

always presents the same face to the Earth. Tidal friction in the Earth-Moon system can also cause the Moon to recede from the Earth and it is possible that the Moon was once much nearer to the Earth than it is now, even possible that it was once part of the Earth. The theory of the effects which tidal friction in such a system as that of the Earth and Moon—a moderate-sized planet accompanied by an exceptionally large satellite and revolving around the Sun—were traced in a masterly manner by Sir G. H. Darwin in a series of memoirs published some thirty years ago. The theory was necessarily coloured to some extent by the then prevalent scientific ideas concerning cosmogony, ideas derived mainly from Laplace's nebular hypothesis. A perfectly natural chain of reasoning leads directly from the discussion of the theory of the tides, through tidal friction, to the most speculative regions of thought as to the origin and evolution of planetary and stellar systems.

EVOLUTION OF PLANETARY SYSTEMS

Until recently Laplace's hypothesis held the field; though the authors of some modern theories might demur to such a description, it still seems fair to describe all the more recent hypotheses as modifications of that propounded by Laplace. Darwin himself broke away somewhat from the Laplacian doctrine when he suggested that the Moon became detached from the Earth as a single satellite and not as a ring. J. H. Jeans broke away still more when he suggested that gravitational instability or the tendency of gravitating matter to concentrate about local nuclei, rather than increased speed of rotation due to cooling, might have been the cause of the disintegration of the primitive nebula into detached masses. But the modern revival of interest in the nebular hypothesis is largely due to the criticisms levelled against it by T. C. Chamberlin and F. R. Moulton and the propounding by them of a view put forward as alternative and named the "planetesimal" hypothesis. In this view the solar system is supposed to have been developed from a spiral nebula, a type of celestial object with which modern telescopes have made us familiar, consisting of a central condensation from opposite parts of which there emanate a pair of spiral arms. Such an object is supposed to have originated from a single star through enormous tidal

forces set up in it by passing near to another star. The assumed partial disintegration thus effected in the parent star and the mode in which subsequently aggregation of the ejected matter into planets and satellites might have taken place offer problems so intricate as to defy calculation; the indications that can be obtained certainly seem to suggest that we have in this theory a modification of Laplace's free from many of the difficulties inherent in his original form.

EARTH TIDES

The body of the Earth, on which the oceans rest, cannot be absolutely rigid. No body is. It must be deformed more or less by the attractions of the Sun and Moon. If we can find out in what manner it is deformed and how much, we can draw inferences in regard to its internal constitution. Thus there arises the problem of *earth-tides*: How can such tides be observed? What conclusions in regard to the state of the matter within the Earth can be drawn from the observations? The movement eludes direct observation. A tide-gauge can record the rise of water above a marked level near a coast and other instruments can do the same thing for the rise above a level measured from the sea-bottom out at sea but the would-be observer of earth-tides has no mark from which to measure. His methods of observation must perforce be indirect. The first attempts were directed to finding the actual height of the so-called fortnightly tide. By the fortnightly tide is meant a minute inequality in the tide-height, having a period of about a fortnight, depending upon the inclination of the Moon's orbit to the plane of the equator. The point at which the Moon is overhead is not always or generally a point on the equator but travels round and round the Earth in a sort of spiral path. The whole spiral lies between two extreme turns, one the most northerly, the other the most southerly, which, however, are not fixed but vary in position from time to time. If we follow the movement of the point, beginning at an instant when it has an extreme northerly position, we find each successive turn of the spiral lying to the south of the preceding turn, until at the end of a fortnight an extreme southerly position is reached. After this the path turns to the north and during the next fortnight each successive turn of the spiral lies to the north of the pre-

ceding turn. This movement of the Moon causes an inequality in the tide-raising force with a period of a fortnight and this inequality in the force affects the observable height of the tide with an inequality of the same period. It is as if, in addition to the tide that comes in twice a day, there were a tiny tide that comes in twice a month. The method of harmonic analysis of tidal observations can draw out from a long series of observations the amount of this tiny tide, just as a suitably tuned resonator can pick out one of the component tones of a musical instrument or of an orchestra. Now the amount which the fortnightly oceanic tide would have if the Earth were absolutely rigid can be calculated. The result that it may be calculated by the so-called "equilibrium theory" was first asserted on insufficient grounds, then denied on the basis of a more rigorous investigation and finally proved by taking account of a circumstance neglected in that investigation. The adventures of this result form a curious chapter in the history of science but must be omitted here. It is now well established. The comparison of the observed and calculated values is one of the methods available for determining the height of earth-tides. Clearly, if the observed value be nearly equal to the calculated, the Earth yields but little; if the observed value be much less than the calculated, the Earth yields a good deal. In the former case it is very stiff or of great rigidity, in the latter the rigidity is small. If the Earth were fluid inside there should be very little fortnightly tide. As a matter of fact, the observed value is nearly two-thirds of the calculated. This result forms an essential part of the famous argument invoked by Lord Kelvin to prove that the Earth cannot consist of a molten fiery core covered over with a thin solid crust.

This argument is greatly strengthened when it is found to be confirmed by others derived from a study of other phenomena than the fortnightly tide. The attraction of the Moon tends to draw a pendulum to one side. The force available for this purpose is not the full amount of the Moon's attraction but the difference between the amounts of this attraction at the centre of the Earth and at the place where the pendulum is hung; and of this difference the horizontal component only can affect the direction in which the pendulum hangs or the apparent vertical. The maximum amount of the available force being only about one eleven-millionth of gravity,

it is necessary to magnify the effect. This is done by using a horizontal pendulum, that is to say, a pendulum free to swing about a nearly vertical axis. The deflexion of the pendulum measures the force acting upon it. If the Earth were absolutely rigid, the Moon would act upon the pendulum with a certain force, as above. It is necessary to take numerous precautions to shield the pendulum from disturbances, such as those due to draughts, to the heating of the soil by the Sun during the day and its cooling at night, even to the tilting of the floor by the weight of the observer. All these difficulties were overcome by Dr. O. Hecker, who installed two horizontal pendulums in an underground chamber at Potsdam and recorded their movements during several years. On analysing his results, he showed that the actual movement of the pendulum is about two-thirds of what it would be if the Earth were absolutely rigid. Hecker's measurement of the lunar deflexion of gravity is a very remarkable achievement. It recalls and evidently confirms the result obtained by analysing tidal observations to pick out the fortnightly tide; and it has itself been confirmed by another series of experiments with horizontal pendulums carried out by Dr. A. Orloff at Dorpat. It is important to note that the deflexion of the pendulum or at least that part of it which is periodic in half a lunar day keeps time with the Moon.

The proper interpretation of these results is a matter of some difficulty. The registering by the horizontal pendulum of a deflexion less than that due to the Moon's force is evidence that it is under the action of other forces which keep time with the Moon; and it is an immediate inference that these forces are due to the deformation of the Earth by the Moon's tide-raising force. This force alters very slightly the shape of the Earth, elongating it towards the Moon and in the opposite direction and flattening it all round at the places where the Moon is near the horizon. The change of shape produces in the supports of instruments a slight tilt and consequently a horizontal pendulum is subjected to a small force which may be described as the "force due to tilting." It is easy to see that the force due to tilting acts against the Moon's tide-raising force. But this is not the only extra force which is exerted on the pendulum. The elongation of the Earth in one direction, combined with the flattening in all perpendicular directions, causes a

change in the attraction of the Earth or a genuine alteration of gravity, due to the attractions of the tidal protuberances and the loss of attraction that accompanies the tidal flattening. This additional force, the genuine alteration of gravity, may be described as the "change of attraction." It is easy to see that it acts so as to reinforce the Moon's force. The observed result is interpreted in the statement that the force due to tilting exceeds the change of attraction by an amount equal to about one-third of the Moon's force. Now if we knew the force due to tilting, we should know how much the surface is tilted and thence how much the Earth yields to the tide-raising forces. If we knew the change of attraction, we could then use the result obtained by observing the deflexion of the horizontal pendulum to infer the force due to tilting and thence, as before, find the amount by which the Earth yields. But the pendulum result will not tell us how much the Earth yields, because all it can possibly give is the difference between two forces; what we want to know is the magnitude of one of them. Observations of the fortnightly tide cannot give us any additional information. They can only tell us what the horizontal pendulum tells us.

The ambiguity of the interpretation to be put upon the results obtained from observations of the fortnightly tide and of the behaviour of horizontal pendulums should make us cautious about accepting statements as to the rigidity of the Earth, when such statements are founded upon observations of these kinds only. It is true that Lord Kelvin proved long ago that, if the Earth were homogeneous and incompressible, it would have to be as rigid as steel to make the observable height of the fortnightly tide as much as two-thirds of that calculated by the equilibrium theory. The fact that the observed height is of about this amount does not enable us to infer that the actual Earth, which is neither homogeneous nor incompressible, is as rigid as a ball of steel. To obtain sufficient evidence for a judgment on this matter it is necessary to have recourse to a different kind of observations and the observations that have proved effective have to do with a phenomenon that has no obvious relation to tides or the lunar deflexion of gravity—the phenomenon of variation of latitude. It has been known for a long time that the latitudes of places on the Earth's surface are not quite fixed or, what comes to

the same thing, that the North and South Poles are not quite fixed points on the Earth's surface. It has become known more recently that the Poles move in irregular paths about mean positions, round which they circulate in a period of about fourteen months. The period which this movement would have if the Earth were an absolutely rigid body is well known to be about ten months; one reason why the actual periodic movement, with a fourteen-months' period, remained so long undiscovered was that observers sought in their records for traces of a ten-months' period. The lengthening of the period from ten months to fourteen is due to the yielding of the Earth. A movement of the Poles means a change of the instantaneous axis of rotation; this is necessarily accompanied by a change in the so-called "centrifugal force." The adjustment of the Earth to rotation about one axis after another involves a deformation, in exactly the same way as if it were subjected to forces which are the differences between the centrifugal force referred to the actual axis and the centrifugal force referred to an axis passing through the mean positions of the Poles. Exactly as in the case of tidal forces, the deformation implies a tilting of the surface and a "change of attraction." The lengthening of the period has been proved to depend upon the change of attraction not upon the tilting of the surface; and the law according to which the change of attraction is connected with the force causing deformation, in the case of variation of latitude the inequality of centrifugal force, has been made out. Further, it has been proved that the law connecting the change of attraction with the force causing deformation must be exactly the same, whether the force in question be an inequality in centrifugal force or a tide-raising force. The result is that from the period of variation of latitude we can infer the change of attraction due to the tide-raising forces.

To determine the actual height of earth-tides it only remains to combine the results of observation in regard to variation of latitude with those of horizontal pendulum experiments. The change of attraction, the force due to tilting, the amount of the deformation have all been determined. But from this information we cannot infer much more about the rigidity of the Earth than that on the whole it is great. It is impossible to fit all the observations by treating the Earth as a body

of one definite rigidity throughout. Being heterogeneous as regards density it may be expected to be so in regard to rigidity as well. It is perhaps not very surprising that it should be possible to fit all the observations by the assumption of a core of greater density enclosed in a crust of smaller density, provided the core be stiffer than the crust; and it is interesting to note that, if the crust be taken to be about 1,000 miles thick and to have the average density of surface rocks, whilst the core is taken to have the density of iron, the average rigidity of the core, computed on the hypothesis of incompressibility, must be nearly three times that of steel, whilst the average rigidity of the crust, computed on the same hypothesis, may be much less than that of steel and indeed less than that of most hard rocks.

RIGIDITY OF THE EARTH

The inference that the greater part of the body of the Earth must be solid and very rigid has been confirmed in a remarkable way by the results of seismological investigations; indeed, the perhaps unexpected conclusion that the inner parts must be more rigid than the outer appears to be required as part of the interpretation of seismic records. The systematic recording by suitable instruments of seismic disturbances transmitted to great distances has been practised for a relatively short time but the results that have been obtained by means of such records have already proved to be of the highest value for Geophysics. When a great earthquake takes place it affects seismographs all over the world; the records always conform to one type, a series of minute tremors being followed by a series of much larger oscillations which subside gradually. When the distinction between the preliminary tremors and the large waves was first noticed, it was supposed by some writers that they were to be classed respectively as longitudinal and transverse waves, in accordance with the well-known physical principle that waves transmitted through an elastic solid body are of two types—waves of compression or rarefaction, characterised by movement parallel to the direction of propagation; and waves of distortion, unaccompanied by change of volume, characterised by movement transverse to the direction of propagation. As the records accumulated and the theory of elasticity

was improved, it was seen that this simple classification could not be maintained. On the one hand it was found that the preliminary tremors arrived at distant places at such times as to indicate direct transmission through the body of the Earth with a nearly constant velocity, whilst the larger waves appeared to be transmitted over the surface of the Earth with a smaller nearly constant velocity. Further, it was found that both the preliminary tremors and the large waves were composite. After the tremors have been going on during a few minutes, a second series of tremors, showing certain characteristic differences from the first, begin to be received, and the result has been established that the movement is mainly longitudinal in the first series, mainly transverse in the second. Both series appear to travel through the body of the Earth with nearly constant velocities. Again it has been found that the large waves present a number of distinct phases, the most important being an initial phase, in which the movement of the ground is mainly horizontal and transverse to the direction of propagation; and a maximum phase, in which the horizontal movement of the ground is mainly parallel to the direction of propagation and is accompanied by considerable vertical movement and a phase of subsidence.

Concurrently with the accumulation of seismic records and the classification of the types of movement which they disclose, there has been a considerable development of the physico-mathematical theory by means of which an account of such movements can be rendered. The first step was the discovery by Lord Rayleigh of a third type of waves. A disturbance set up in a solid body spreads out in a composite wave, which gradually resolves itself into two waves, one of compression, the other of distortion, with a peculiar type of motion between the two. When the front of a wave reaches a bounding surface reflexion takes place; the reflected waves are in general composite at first and resolve themselves gradually into pairs of waves of the two special types. The effect of a bounding surface is, therefore to produce changes which may disguise the simplicity of the resolution into the two types; the result which Lord Rayleigh found was that disturbances emerging at the surface give rise to a distinct class of waves, which travel over the surface with a nearly constant velocity and never affect appreciably the matter at any considerable depth beneath the

surface. Waves of this type are characterised by a horizontal movement parallel to the direction of propagation, accompanied by considerable vertical movement. The conclusion that the maximum phase of seismic movement must be transmitted by waves of this type seems inevitable. The phase of subsidence might be supposed to be due to the frittering away of the energy through internal friction; doubtless this cause plays a part but it has been proved by Prof. H. Lamb that waves which spread over a surface, as distinguished from waves which travel through a body, are always prolonged in a kind of "tail," showing a gradual diminution of intensity, quite independently of any dissipation of the energy. The characteristic feature of the initial phase of the large waves, viz. the transversality of the horizontal displacement, can be explained only by taking account of the heterogeneity of the Earth's substance. Waves possessing this feature can travel through a superficial layer, provided the rigidity of the subjacent material be greater than that of the layer.

By regarding the Earth as made up of a nucleus and a moderately thick superficial layer or crust and attributing suitable mechanical properties to the nucleus and to the crust, we can arrive at a fairly consistent representation of the various phenomena. The first and second phases of the preliminary tremors are, in this representation, taken to be due respectively to compressional and distortional waves which travel through the body of the Earth and emerge at the surface. The initial phase of the large waves is taken to indicate the passage of waves of transverse horizontal displacement transmitted through the crust; the maximum phase to indicate the passage over the surface of waves of Lord Rayleigh's type; and the phase of subsidence to be the expression of the tails in which both these types of waves would necessarily be prolonged. The values to be attributed to the physical quantities by which the state of the parts is specified are not completely determinate, a change in the assumed density, for instance, being accompanied by a change in the inferred rigidity. But the indeterminateness is confined within relatively narrow limits. The order of magnitude of the rigidity required in the nucleus or at least in its more central portion is about three times the rigidity of steel. This value may seem very large; but, when we reflect upon the enormous pressures which must be

developed within the Earth by the mutual gravitation of its parts, it becomes less surprising. A similar value was inferred by combining the result of horizontal pendulum experiments with the result of observations concerning variation of latitude. The value required in the crust is about the average rigidity of many kinds of granite and marble. The result that, for the proper transmission of the initial phase of the large waves, the rigidity should increase beneath the crust, points to a gradual transition from the mechanical properties of the crust to those of the nucleus, a thing probable enough. The general result that the Earth as a whole is a very rigid body, not a fluid body coated over with a thin solid crust, is so well supported by the observations of the fortnightly tide, by the experiments with horizontal pendulums, by the period of the variation of latitude and by the interpretation of seismic records, that it should by now be regarded as firmly established.

DR. PAVY AND DIABETES

By F. GOWLAND HOPKINS, M.A., M.B., D.Sc., F.R.S.

THE death of Frederick William Pavy at the age of eighty-two closed a remarkable career. It is not often that an exceedingly busy professional man retains unimpaired, throughout a long life, a vivid interest in the purely theoretical side of the problems of his profession; more usually intellectual relief is sought in other fields. An eminent physician is rarely found busy at once in practice and in the laboratory; less often still are such combined activities exercised over considerably more than half a century; and it is, I think, even more rarely that a scientific worker, of any sort, is found content in his old age to struggle with just those elusive and somewhat limited issues which occupied him at the beginning of his career. The scientific veteran usually comes to crave a more extensive area of action; if he have not left science for philosophy or affairs, his interests are usually concerned with the wider aspects of his subject. But Pavy, with fourscore years behind him and still a busy consultant ever remained an active laboratory worker, faithful to his original quest and as keen an inquirer as in his youth.

A few months before his death, he wrote to Prof. Armstrong in the optimistic spirit which was characteristic of him. "My great object, before life comes to an end," he says, "is to elicit all the useful knowledge I can bearing upon Diabetes"; and he speaks of his faith in the reality of the progress being made. His latest colleague in research, Mr. Godden, tells me that, to the very end, he would seize available moments between the morning visits of his patients to enter his laboratory and watch the progress of experiments. His afternoons were spent in continuous experimental work at the physiological laboratories of the London University and this routine was continued to within a week of his last vacation, from which he returned with but nine days of life left to him.

Pavy was born in 1829. Educated at the Merchant Taylors' School, he entered as a student of Guy's Hospital in 1847. In

1853 he obtained his Doctorate at the London University; his first paper, entitled, "Saccharine matter: its physiological relations in the animal mechanism," was published in the Guy's Hospital Reports of the same year. It was the precursor of some two-score of papers; the last of these has but just appeared and was published posthumously. Almost any one of the long series might well have received the title of the first. Pavy's scientific interests were indeed in one way extremely circumscribed: though other aspects of medicine and physiology received attention from him intermittently, the subject which really absorbed him was the metabolism of carbohydrates—normal and erratic. Adolescent or aged, he remained devoted to the problems of this domain. He was, of course, a specialist in the treatment of diabetes and his professional fame ensured him, during half a century, a lucrative consulting practice. But it must not be supposed either that his interest in the metabolism of carbohydrates arose merely from his professional needs or that his labours as an investigator had anything whatever to do with the desire to advertise his special knowledge: both began before his practice took shape; both lasted without abatement long after his practice needed any prop whatever.

He once told me himself that it was an instinctive interest in this particular aspect of physiology that led him to specialise professionally. He was a great believer (as who should not be?) in the value of pathological studies to the physiologist and was apt to think that if physiologists could see as much as he himself had seen of human diabetes they would more readily accept his teachings concerning the normal fate of sugar in the body.

It must be admitted that Pavy's special views did not, as a matter of fact, conform to current opinion. In discussing them I shall be bound, as a result of my own predilections, to take more or less the standpoint of orthodoxy. But I write as one personally indebted to the stimulus of Pavy's teachings and as one who has seen physiology gain more from Pavy's work and enthusiasm than from the writings of many who have kept step with the majority.

CLAUDE BERNARD'S GLYCOGENIC HYPOTHESIS

During the year in which he took his degree, Pavy paid a visit to Paris; there he met Claude Bernard. Some few

years earlier, this great physiologist had published his account of the experiments which established belief in what is generally known as the glycogenic function of the liver; Pavy, during his visit, doubtless received an account of the work at first hand.

It is well to be clear with regard to the exact use of the expression "glycogenic" as at first applied to the functions of the liver. Bernard had set himself to explore the organs of the body in the endeavour to locate the regions in which sugar is utilised and destroyed. His hope was to discover what deficiency might be responsible for the condition of diabetes and by mitigating that deficiency to effect a cure of the disease. He already knew that all carbohydrate food leaves the intestine in the form of dextrose. The liver is an organ standing in the path of transference from the intestine to the tissues and Bernard first sought evidence for the destruction of the dextrose in that organ. He found, however, that sugar was present in the blood of the hepatic veins immediately beyond the liver during the absorption of carbohydrate from the intestine. He then discovered something more striking: that when the animal was not taking carbohydrate but consuming flesh alone—no sugar, therefore, flowing from gut to liver—sugar was still to be found leaving the latter continuously and passing into the general circulation beyond it. Claude Bernard held, therefore, that sugar must be actually made in the liver.

All this was before he had discovered the nature of the actual precursor of the sugar which leaves the liver and he conceived at this time that the organ elaborated carbohydrate from material which was not carbohydrate; actually it "secreted" sugar and was in a literal sense of the word "glycogenic." Later, however, Bernard discovered that the precursor—at all events, the main precursor—of hepatic sugar was itself a carbohydrate, a polymerised sugar, in fact, which easily gave rise to sugar under simple treatment. Its discoverer recognised the physiological analogy of this substance with another complex carbohydrate—the starch of plants—and though now known as glycogen it has often been called "animal starch." The term "glycogenic," as applied to the liver, now took on a somewhat different aspect; the organ is not in the main concerned in the production of carbohydrate *de novo* but is a particularly capacious storehouse of carbohydrate awaiting utilisation. Its

store of glycogen can be filled up when sugar is flowing from the intestine and then drawn upon when demands arise, the glycogen being reconverted into sugar and transported in this form to the seats of utilisation.¹

Bernard's discoveries thus provided physiology with a clear and simple view concerning one fundamental aspect of the metabolism of carbohydrates and this view is one which still claims the suffrages of almost all. The facts in support of it seem now, as we shall see, more cogent than they did to Bernard's contemporaries.

PAVY'S ANTAGONISM TO THE GLYCOGENIC HYPOTHESIS

Yet Pavy dissented wholly from Bernard's point of view. Every word that he spoke or wrote concerning the metabolism of carbohydrates emphasised his antagonism to it; throughout his life he was engaged in marshalling facts which, in his belief, proved it to be in error. To understand his teaching and the drift of his work, it is very necessary to appreciate this antagonism and how it arose. Before tracing its origin, however, it may be well to point out that Pavy's earlier views, though they remained intact until quite the final period of his life, were modified ultimately not a little by contact with the work of others. Those who knew him ultimately are well aware that during a long period he read but little of the current literature. He was impatient of the dominance of Bernard's views on the Continent and discussed his work but little with his colleagues save when engaged in actual polemics (which, it must be confessed, were somewhat of a joy to him).

When still upon the active staff of Guy's, his purely scientific work was confined within the four walls of his laboratory there. Subsequently (in the later nineties), when working at the laboratories of the Colleges of Physicians and Surgeons,

¹ It might be termed therefore a "glycotactic," or, much more accurately, a "glycodianomic organ (*διανέμω*). My colleague, Mr. E. Harrison of Trinity College, who suggested the latter word, tells me that Plato speaks somewhere of the lungs as the "Stewards of the Winds." "Steward of the Sugar" would so exactly express the nature of the function of the liver that but for fear of pedantry one would be inclined to call it "glycotamieutic."

he came under influences ¹ which led him to familiarise himself with the work of others.

His attitude at the last, as displayed, for instance, in the lectures delivered before the Royal College of Physicians in 1908, was further removed from his own earlier views and nearer to that of the majority than he himself seemed to realise. He developed, indeed, in these later years, a knack of weaving a new web of facts into the warp of his older views while gradually removing the less durable threads from the latter. In the end, though he seemed unable or unwilling to recognise it, the material had become of almost orthodox pattern. He, at any rate, believed to the very end that he had disproved Bernard's original views and all that was based directly upon them. His antagonism to the glycogenic doctrine was still strongly expressed in his last published lectures.

I propose now to examine the reasons for this antagonism. It was due in part to the interpretation he put upon his own earliest experimental researches but more, I think, to the fact that two preconceptions dominated his mind: one respecting the nature of the renal functions, the other concerning the fundamental nature of animal metabolism as a whole. Each of these factors may be considered in turn.

Very shortly after his return from Paris, Pavy began to work upon the carbohydrate question and was led to estimate the amount of sugar in the blood of the right ventricle of the heart when obtained from the living animal. He found—and was greatly impressed by the observation—that, as a matter of fact, in life, this blood did not carry the excess of sugar which Bernard had shown it might contain postmortem nor that which apparently it ought to contain according to the glycogenic doctrine. This led him to suspect that the supposed excess of sugar in the liver was due to post-mortem changes. After developing a technique for the avoidance of such changes, he showed experimentally that there was no excess in the organ: that if such be ever observed, it is only when the liver has suffered damage at the hands of the operator. The glycogenic doctrine ²

¹ I happen to know that in later life he was most grateful to Prof. Brodie and to his own private assistant and colleague in research, Mr. Siau, for breaking down at this time the habit he had acquired of isolating himself intellectually.

² In its later form, that is to say. When Bernard first initiated it, the seat of oxidation was supposed to be in the lungs.

further postulates that since sugar is transported from the liver to the muscles and other tissues, where its oxidation takes place, arterial blood should contain more sugar than venous. Pavy's estimations failed to show such excess.

But if the liver contain no more sugar than other organs and yield no sugar to the blood leaving it, if there be no transport of sugar as such to the tissues, the glycogenic explanation fails. Pavy felt that his researches proved all these negations and, as I have said, disbelieved profoundly in Bernard's views to the end of his life. His disbelief was supported by an argument which for him was conclusive. Normal blood throughout the body contains always a certain small proportion of sugar (about one part in a thousand) and normal urine also contains a definite though small amount. These circumstances have been amply demonstrated by many observers but Pavy himself took much trouble to obtain accurate quantitative data, both from blood and urine. Now, in his view, any variation in the amount of sugar in the former must be promptly indicated by a corresponding variation in that of the latter. He held it was impossible that a diffusible substance, such as sugar, with its relatively small molecules, could fail to pass the kidney in proportion to its concentration in the blood. But as he pointed out, no such variations can be detected in the case of the healthy person. At no time after a meal of carbohydrate is the condition of the urine such as to indicate an increased excretion of sugar; therefore the constituents of that meal can never enter into general circulation in the form of sugar.

In his criticism on the experimental work which was supposed to support the glycogenic hypothesis by demonstrating a special distribution of sugar in the circulation, Pavy was upon strong ground. His own researches, even the earlier, were made with the aid of better methods and in a more critical spirit than those of his predecessors. If a belief that the liver operates as a storehouse of available carbohydrates must be based on the proof that on occasion sugar passes from it into the blood, in such quantity that it may be detected analytically, Pavy's work deprived that belief of foundation. Those who still hold it are content to point out that the flow of blood from liver to tissues is so rapid that the transport of large quantities of sugar need cause but an infinitesimal percentage increase in the sample drawn off by the experimentalist

for analysis, an increase which may well fall within the limits of experimental error. Whilst, therefore, an experimental proof of Bernard's theory cannot be obtained on these lines, a disproof is equally impossible.

The arguments which Pavy based upon his view of the renal function, though they seemed to him to appeal to common sense and to be conclusive, were, on the other hand, essentially *a priori*. That no increase of sugar takes place normally in the urine as the result of a carbohydrate meal merely demonstrates the perfection of the regulative activity of the liver: the organ maintains the concentration of blood-sugar at a value near to a mean, in spite of great fluctuations in the supply from the intestine. On the glycogenic doctrine, fluctuations in the demands of the tissues would, it is true, involve a fluctuating output of sugar from the liver and any such fluctuations, Pavy assumed, should be promptly registered by the kidney. This assumption is not wholly valid, however. Increased demands for sugar in individual organs may be met, in part or whole, by increased velocity in the local blood flow rather than by increased concentration of sugar in the blood. In many cases, again, increase in the oxidation processes of the tissues in general is associated with increased activity in the kidney itself (*e.g.* in the adjustment of the body following a lowering of external temperature) and this organ is one with a high-grade metabolism, likely to utilise rather than to excrete any temporary excess of sugar which passes it. Finally, though we know that the kidney is extremely sensitive to increases of sugar in the blood of above a certain amount, it is more than a mere filter and we do not know that such small variations as might be sufficient to cover the fluctuating demands of the tissues would be registered in it.¹

It is striking to find that a direct proof that sugar may increase in the circulation without glycosuria, far more convincing than such considerations as the above, was to be furnished by the very last of Pavy's own work which came to publication. In conjunction with Mr. Godden,² he injected sugar into the venous circulation of rabbits—under conditions which were more physiological than those of earlier experiments of the same type; and found that no less than 2 grammes of

¹ Cf. E. Frank, *Zeitsch. f. Physiol. Chem.* 70, 291 (1911).

² Pavy and Godden, *Journ. Physiol.* xliii, 199 (1911).

dextrose per kilogramme of body-weight could be injected in the course of fifty-five minutes without any trace of glycosuria being noticeable. If we may transfer such figures to the case of a man of average weight (70 kilos.) they mean that more than 150 grammes of sugar per hour or 3,600 grammes a day, at least seven times the normal consumption of carbohydrate, might enter the circulation without appearing in the urine. To say the truth, such figures are startling and require further investigation to explain them. It is hardly likely that they can be legitimately applied to human physiology but they leave, at any rate, a large margin of evidence on which to base our belief that hepatic sugar may enter the circulation normally in quantities sufficient to supply the maximum demands of the tissues without inducing glycosuria as a necessary consequence. How far Pavy would have adjusted his teaching to meet these results, which were only published after his death, we cannot tell; as all his writings show how great was the importance he attached to an argument which his very last experiments were to undermine, the circumstances are not without a degree of pathos.

PAVY'S OWN HYPOTHESIS CONCERNING ASSIMILATION: THE LYMPHOCYTES AS CARRIERS OF THE FOOD

To return to the discussion of his published views. If the glycogenic hypothesis be wrong and sugar be not transported from liver to tissues, if therefore the glycogen found in the former be not a store of carbohydrate to be drawn upon by the latter, what is the significance of its appearance after carbohydrate has been consumed? Being an insoluble, non-diffusible form of carbohydrate, the formation of glycogen provides the chemical mechanism for trapping the intestinal sugar which must be prevented from entering the general circulation. When, according to Pavy's earliest teaching, it disappears from the liver, it undergoes constructive, not destructive, changes. That sugar can be converted into fat in the body is a physiological certitude and Pavy's original conception was that the formation of glycogen was essentially the first step on the way to such conversion. He was prepared, however, to believe that some other assimilative path might be open to it; what he felt to be certain was that it was never again broken down into sugar. As his views developed they became more definite with

regard to the immediate fate of carbohydrate in the body. His teaching became even more dogmatic than before on the point that the body must protect itself from the circulation of free sugar; he displayed, moreover, as was only logical on his part, a strong antagonism to the current idea that protein enters the blood broken down into its constituent amino-acids. It seemed to him obvious that anything added to the blood in such a way as to disturb its mean composition must circulate in large molecular complexes, insusceptible of leaking through the kidneys. He held, therefore, that the foodstuffs were "assimilated" at the very earliest stage of their entry into the body.

Two cellular mechanisms guard the portals of entry: fixed epithelium cells, which line the intestinal wall; free floating cells (lymphocytes), which normally crowd the lymph spaces of the intestinal villi but are susceptible of being transported through lymphatic channels into the blood. Orthodox physiology attaches many functions to the epithelium cells and among them some of a synthetic nature. Pavy added another function. He believed them to be capable of converting sugar directly into fat and looked upon them as constituting the first line of defence possessed by the body against the entry of diffusible sugar. He held that he had actually seen this immediate conversion of carbohydrate into fat in the intestinal wall of the rabbit, though his observation, it must be confessed, is not easy to repeat. Later on he attached much more weight to the functions of the lymphocytes; reading his later writings in the order of their appearance, one realises that his faith in the assimilative importance of these cells became more and more vivid. In his last years, indeed, he found it difficult to understand how any one could disagree with him on this point. His faith certainly went far. Others have looked upon the lymphocytes as important agents in the transport of protein from the gut but Pavy took a bolder view: he conceived that all the protein and carbohydrate eaten, all the supply meant for the tissues as a whole, is first assimilated by the lymphocytes; only when there is marked excess of food to be dealt with is the function of the liver as a second line of defence necessarily called upon. This assimilation by the lymphocytes is of the completest kind, leading to an actual growth of the cells, proportionate to the amount of food absorbed; even as yeast-

cells grow upon a medium of sugar and nitrogenous matter, so do the lymph-cells develop upon the sugar and peptone provided by the intestine.

"Food that has been broken down and placed in a fit state by digestion for absorption is at once dealt with at the seat of absorption and rebuilt into an elaborated form. Dextrose and peptone are alike recognisable at the seat of absorption but both thereafter disappear. At the same time and at the same spot, there is an active bioplasmic growth taking place and bioplasm is known to feed upon dextrose and upon peptone. The lymphocytes which constitute the growing material can be followed from the villi into the absorbent vessels and thence through the thoracic duct into the vascular system."¹ Once in the blood, the lymphocytes are broken down into indiffusible products which become available for the nutrition of the tissues generally.

However startling and at variance with the trend of modern physiological thought it may be, a theory propounded by so acute a thinker must not be dismissed without examination. Increase of lymphocytes in the blood, as a result of food digestion, is a phenomenon long known and well established. It is quantitative considerations alone which make Pavy's theory difficult of acceptance. It is not, maybe, altogether unthinkable that cells of the type of white blood corpuscles should increase at the great rate postulated by the theory. In the case of unicellular organisms multiplying by fission great rates of increase have been observed when the conditions for growth are favourable. But histologically the lymph-cell does not by any means present in its nucleus the characters which are associated with the process of rapid growth; moreover considerations of the quantities involved, even though we can only estimate them approximately, seem to make the theory quite inconsistent with the facts observed in the animal. An adult man in the course of twenty-four hours eats some 600* grammes of protein and carbohydrate taken together. An animal cell contains not less than 75 per cent. of water, so that the actual mass of lymphocytic protoplasm that would be formed from the day's dietary on Pavy's view would weigh perhaps 2½ kilogrammes. Now a calculation indicates that the total mass of white cells in the blood under average conditions

¹ See the *Lancet*, 1908, II. 1584.

is of the order of from 5 to 6 grammes, so that if the daily flow of lymph from the intestine really brought so large a mass of lymphocytes into the blood, either the rate at which they are destroyed must be almost inconceivably rapid or else a meal would increase their numbers to a degree out of all proportion to that observed.¹

Pavy himself made an important discovery, which he felt made it easy to believe in the temporary disappearance of the carbohydrate of the day's diet in the bioplasm of lymphocytes. Such a cell consists normally in the main of protein; Pavy, however, found that a carbohydrate constituent is always contained in the molecules of proteins. We are to see that this is a fact with qualifications; but neglecting these for a moment, it must be remembered that the protein in the diet, which has to be assimilated, already contains its own proper proportion of carbohydrate and a proper proportion of carbon and nitrogen. To this, if we read our author literally, the growing bioplasm of the intestinal leucocyte adds all the carbohydrate of a mixed dietary, so that its composition as it enters the blood-stream must be very different from anything met with in a normal animal cell: the proteins of the lymphocyte must contain some three or four per cent. of nitrogen only, instead of fifteen or sixteen per cent. Otherwise it must proceed from the intestine loaded with glycogen, a condition which Pavy does not predicate and which histological examination disproves.

I have assumed in the last few paragraphs, because Pavy appears to assume it, that the lymphocyte could assimilate all the material from the intestine and arrive in the blood with the supply intact. This could not be the case actually; considerations respecting energy make it impossible. The growth of living cells can never go on in such a way that the total energy of the material consumed during growth is stored in the material built up. Such rapid growth and destruction of cells as the hypothesis under discussion calls for is of such an exceptional kind that we have no data upon which to base an estimation of the energy changes likely to be involved; but it is certain, I think, that the process would involve a liberation of energy during the period in which food is absorbed out of all proportion to that actually observed.

¹ Cf. Halliburton, *Lancet*, 1909, Jan. 2.

Such objections as I have urged against the lymphocyte hypothesis of assimilation either did not occur to its author or had no weight with him. In 1906 he states the matter dogmatically thus: "Food into lymphocytes, lymphocytes into proteids, proteids into tissue-substance may be taken as representing the chain of physiological connexions between the food and the tissues."

LATER ADJUSTMENTS IN PAVY'S VIEWS

I have spoken above of two preconceptions dominating Pavy's mind. The first, concerning the relations of the blood and the kidneys, has been dealt with; the second was one which made it possible for him to hold a view such as that just discussed. He was one of those who held that chemical changes in the material of the animal body occur only while such material is in the strictest sense a part of the living complex. The molecules that undergo change are molecules that are in some way alive. Such an assumption involves either a tendency to cease thinking about the phenomenon in terms of structural organic chemistry altogether or a tendency to use loose pseudo-chemical concepts of "living molecules with stable central nuclei and active side-chains." This is not the place to discuss so difficult a question as the chemical constitution of bioplasm but it is important to point out that the encouraging recent progress in biochemistry has been associated with a recognition of the fact that the complex tangle of chemical interactions involved in life is susceptible of some experimental analysis into separate interactions which may be studied by purely chemical methods; secondly (at least to the minds of many), with a steady faith that full acquaintance with such separate interactions will ultimately be followed by some knowledge of the manner in which they are co-ordinated in the bioplasm. There are, indeed, chemical happenings in the living cell itself which are to be looked upon as isolated from the bioplasm, interactions which may be termed interplasmic rather than intraplasmic. When an amoeba has ingested food material, the digestive processes which go on, though intracellular, are strictly interplasmic in the sense mentioned, as it is to be supposed that suitable enzymes become operative in the vacuole with which the food particle is quickly sur-

rounded. Now, be it noted, it by no means follows that, even in the case of the amoeba, all the digested food material is "assimilated." Assimilation in the strict sense may, in the case of ingested protein, for instance, be a highly selective process, even in unicellular organisms and other chemical changes may follow mere hydrolysis in the vacuole. It is clear that interactions may occur in interplasmic spaces less obvious to the microscope than the large but temporary food vacuole of the amoeba; it has been boldly suggested by Hofmeister, in fact, that a tissue-cell may be a laboratory in which a great number of isolated interactions precede, each in its own locality. The colloid nature of the medium and indiffusibility of specific enzymes secure the localisation and independence of the individual interactions. Such a view may go too far and it cannot be claimed that physiological thought has yet clarified itself in connexion with such matters but it is of importance to recognise there is no necessity to assume that "dead" matter must become "living" matter before it suffers biochemical change. In a case which specially interests us at the moment, that of sugar in its relation to glycogen, there is full justification for the belief that the conversion of either into the other involves no merging into an unknown complex of bioplasm but only the progress in the one direction or the other of a simple reversible interaction conditioned by a specific enzyme. That some property of the cell controls the direction of the interaction in a manner that is largely unknown is a fact which must be admitted.

If we now consider Pavy's later teaching as to the part played by the liver in the metabolism of carbohydrates, we shall meet with an illustration of his more or less unconscious adjustment to modern views. As already stated, he came to think that when the intestinal mechanism is normal the liver plays but a subordinate part in arresting unassimilated sugar. It forms glycogen just as other organs form glycogen from the complexes containing carbohydrate brought to it by the blood. Because of its position and special activities it forms proportionately more of this substance than do other organs. When in 1894 he wrote his *Physiology of Carbohydrates*, he had come to speak of the hepatic glycogen as a "store" of carbohydrate; but, at this stage, he still appeared to view it as stored by the liver for its own purposes, just as a yeast-cell

or a muscle-fibre stores it. But by the time *Carbohydrate Metabolism and Diabetes* was written (1906) he had come nearer to Claude Bernard. "The seat of actual consumption is in the muscles and therefore, in the case of its disappearance (*i.e.* the disappearance of glycogen) from the liver, there must be transport in some way or other through the circulating system," though the transport is not in the form of free sugar. A change from time to time in the language he uses when discussing the action of the liver-cell illustrates the gradual modification of his views. At one time we find only such statements as that the bioplasmic complex of the cell "takes on" sugar and "gives out" glycogen or fat. It might again "take on" glycogen and "give out" fat; only in the case of the liver bioplasm there is no "giving out" of sugar. At this time, in common with all or most writers, he made a sharp distinction between the powers of enzymes which could only bring about degradations and those of the protoplasm itself, which could induce synthetic and constructive changes. He was clear at that time that the production of sugar observed in the excised liver was due to the influence of an enzyme exercising an activity which was essentially a post-mortem phenomenon of no importance physiologically. In 1897-8 he was engaged in a controversy on this point, in which he showed, as always, great dialectical skill, from which, it must be confessed, he emerged victorious as an experimentalist. But neither he nor many others then realised what Goethe appears to have realised when he wrote in Wilhelm Meister's Lehrjahren, "Nach dem Tode arbeiten sich die Kräfte, die vergebens nach ihren alten Bestimmungen zu wirken suchen, ab an der zerstörung der Teile die sie sonst belebten."¹

Pavy was after all as ready as most to realise later on, when experimental work had clarified our views, that the enzymes which, after disorganisation of the tissues containing them, produce results that are, quantitatively at any rate, unphysiological, may be agents which "animate" the tissues when their work is duly organised and orientated in intact cells. We find him (*Carbohydrate Metabolism and Diabetes*, p. 68) fully admitting subsequently that the process which precedes transport of carbohydrate from liver to tissues is saccharification of the glycogen by the diastatic enzyme; only, once more, the sugar must not be

¹ Quoted by M. Jacoby, *Ergabenisse der Physiologie*, I. i. p. 239.

supposed to wander in a state of freedom. As to the mechanism of its transport, his views also showed developments. At first, as we have seen, he denied the possibility of transport altogether but in 1893 came his own discovery of what he termed the "glucoside constitution of proteid matter," which modified his views. It had been suspected at an earlier date that the protein molecule yields something of a carbohydrate nature upon hydrolysis and the work of Schützenberger had given support to the belief. But Pavy came upon the fact independently and his observations were more exact and went much further than those which preceded them. They opened indeed an interesting and important chapter in biochemistry. It was shown that among the products of the complete hydrolyses of protein was a substance yielding a characteristic crystalline derivative identical with the osazone of dextrose. The quantity of this substance which can be obtained from ovalbumen, the protein chiefly worked with, was considerable. Here, then, felt Pavy, is the form in which sugar may be transported in the blood without possibility of loss by way of the kidney. He came, indeed, to attach the greatest importance to this protein-sugar, not only in relation to transport but in connexion with other and more general phenomena of the metabolism of carbohydrates. Quotations (1906) will define his position both with regard to the mobilisation of liver glycogen and its transport. The suggestion, he says, "presents itself that sugar is taken on as a side-chain by a proteid constituent of the blood and transported to the tissues where it is taken off for subjection to utilisation"; and then later, "Glycogen is a storage material consisting of very large molecules and therefore not adapted for shifting its position. I should think that the first action that occurs is the breaking down of its molecule into molecules of glycose which become instantly taken on by the alluded-to molecules of the blood. There may be concerted action between the breaking-down and taking-on processes but that there is such an operation is rendered probable by the fact that there is no show of sugar in connexion with the occurrence. Enzyme action, it may be considered, of necessity constitutes a part of the process. . . ."

But further study on the part of others showed that the facts of the case are not quite such as can support these views with regard to transport, at least, not in the definite sense in which

they are formulated. Proteins are not glucosides. In the first place, the group present in their molecule is not strictly a carbohydrate group. What is really obtained on hydrolysis is a nitrogenous derivative of dextrose (glucosamine). This substance contains an amino-group and so bears a relation to the other constituents of protein—the amino-acids. Its constitution is such that it yields an osazone identical with that given by dextrose, so that the evidence relied upon by Pavy to prove the production of the latter was misleading. The substance is not yielded by all proteins and is probably absent from the molecules of typical blood proteins, the amount obtainable from the serum-albumen being so small as to suggest that it arises from some impurity. What is of special weight against Pavy's views as regards its significance is the fact that glucosamine does not behave as a carbohydrate in the body; it yields, for instance, no glycogen to the liver.

With regard to transport, we find that, at the end, Pavy was willing to simplify his views. He had been struck by a paper by Bayliss dealing with "adsorption" as a preliminary step to chemical action and seems to have decided that the existence of a loose compound of circulating sugar with the blood proteins will account for the failure of the latter to be excreted.¹ In the paper already mentioned as published after his death he wrote: "After adsorption taking place, the sugar, whilst recoverable (from the blood) by analysis, would be virtually holding a colloidal position and in this state would escape being eliminated by the kidney."

I think it must be admitted that Pavy's final position with regard to the function of the liver in the metabolism of carbohydrates does not differ vitally from that of Claude Bernard nor that of the present-day majority. He held, it is true, that the liver does not deal with all the sugar absorbed from the intestines but only with a part of it. He came to admit, however, that the hepatic glycogen arises directly from the carbohydrate of food, that it represents a store held in trust for the tissues and that it is mobilised for transport by an enzyme which converts it into sugar. The added view that during transport it is not strictly free but forms a loose compound with the blood proteins does not carry us far from the teaching of

¹ The view had been previously advanced by Otto Loewi, *Archiv für exp. Path. und Pharm.* xlviii. 410 (1902).

Bernard, who, of course, had no reasons in his day to consider such possibilities. It is by no means surprising that an investigator's views should be modified with the process of time but it is striking to find that Pavy, in spite of his modified attitude towards the facts, held to the end, as his latest writings show, that the glycogenic doctrine is "mischievous." If in any sense it be so, it is clearly not because it is in essence wrong but because, as originally formulated and as generally understood, it allots to the liver too large a share in the initial stages of the metabolism of carbohydrates. When the matter is narrowed down to this quantitative issue, Pavy's views are seen to be special and, maybe (even if we cannot admit the lymphocyte theory), are also right.

THE UTILISATION OF SUGAR IN THE BODY

I have so far dealt with one aspect alone of the metabolism of carbohydrates and have only discussed the fate of carbohydrate of the body before its utilisation, as a source of energy or otherwise, has begun. Of the processes associated with utilisation nothing has been said. When the views of Pavy are under discussion, the attention is inevitably directed more particularly to these earlier stages of metabolism, because he was himself almost entirely preoccupied with them. He was concerned to explain the nature of diabetes and he held that the abnormality producing that disease was to be sought among the anabolic or assimilative stages of metabolism. In the diabetic organism, he held, catabolic and oxidative processes may be wholly normal. To him the question of right or wrong in the metabolism of carbohydrates was in its broadest aspects a simple one: the normal body converts its carbohydrates into complexes immediately it receives them and sugar never circulates as such. In the errant organism the initial synthetic assimilative functions fail, sugar circulates as such and passes the kidney and this circumstance constitutes the essence of the diabetic condition. The general view is more comprehensive. The error, it is held, may also be on the other side of metabolism; the body may be diabetic because it fails to grip its sugar at the locus of utilisation. In any case, we have to consider that other region of metabolism.

In 1889 von Mering and Minkowski presented a gift to

physiology and pathology the great value of which they have recognised though they have both experienced great difficulty in learning how exactly to use it. These experimentalists removed the pancreas from dogs and showed that its removal is followed at once by a permanent condition of glycosuria. We now realise fully that in the absence of some pancreatic function the power to utilise sugar is completely absent from the tissues. What exactly is the nature of that function? In spite of much endeavour the answer to this question is far from complete. The general opinion, at any rate, is that it is exercised at the seat of utilisation. An objective view is taken and seems justified by experiment, that when an active tissue element is to abstract energy from sugar, a *tertium quid* is necessary to enable the former to get its chemical grip upon the latter. This *tertium quid* is supplied in the internal secretion of the pancreas and reaches the tissues by way of the blood. Perhaps because scientific thought tends to run in ready-made channels but also because of some experimental justification, this view is made more definite by attributing to the pancreatic factor the functions of an "amboceptor"—a conception and a term derived from the literature of immunity. An amboceptor is an agent which, by its ability to combine chemically with each of two substances incapable of combining when alone, completes a chemical system in such a way that the two substances are brought into interaction. This is essentially a definition of a catalyst but the action of an amboceptor has certain quantitative relations, which I must not stop to define more closely, which put it in a special class of catalysts. A current conception is that the pancreatic amboceptor brings some enzymic mechanism of the tissues into relation with the sugar. Until quite lately, at least, the evidence seemed to show that it was directly concerned with the breakdown of sugar. Pavy, when he came, somewhat late in the course of his teaching, to express views as to the influence of the pancreas, accepted the term amboceptor but modified the conception of its action in a manner which was characteristic. It is, according to him, an agent necessary for assimilation; only in its presence can sugar be so linked on to bioplasm as to undergo ultimately the necessary building up into the living complex of lymphocytes or liver-cells. In this connexion he himself carried out experiments which showed that when pancreatic extracts are injected

into the circulation, simultaneously with sugar, there is an increase of what he termed the "amylose" carbohydrate of the blood, a more complex substance than sugar itself. This pointed to an influence upon synthetic rather than upon destructive or oxidative changes. Now some confirmation of these results has recently been obtained. When pancreatic tissue is ground up with muscle tissue, better still, when an alcoholic extract of boiled pancreas is mixed with muscle plasm and dextrose is added, the sugar disappears from the mixture with considerable rapidity.¹ The disappearance either does not occur or occurs much more slowly, when the dextrose is in contact either with muscle alone or with pancreas alone. What has been actually observed in such experiments is a diminution in reducing power and this has always been interpreted as meaning that the sugar undergoes destruction. But it has been shown quite lately that, as a matter of fact, the disappearance of the dextrose is due to its condensation into a more complex sugar having a smaller reducing power.² Here then, we find, at least in a limited sense, a confirmation of Pavy's contentions; for if experiments such as those described really bear upon the physiological happenings in the body, a synthesis of some sort would seem to precede the utilisation of sugar by the tissues.³

Further inquiries into this point will lead us to consider the more purely chemical side of the whole question and that very small modicum of knowledge concerning it which can be discussed in terms of molecular structure.

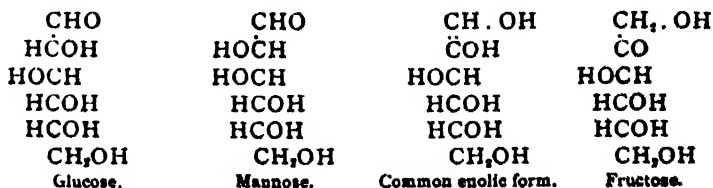
It must not be forgotten that though dextrose or grape sugar is by far the most prominent physiological sugar, the animal

¹ Otto Cohnheim, *Zeitsch.* xliii. 401 (1904); *ib.* xlvii. 253 (1906). Also Hall, *Amer. Journ. of Physiol.* xviii. 283 (1907).

² Levene and Meyer, *Journ. Biol. Chem.* ix. 97 (1911).

³ Since the above was written, Knowlton and Starling have published (*Proc. Roy. Soc.* lxxxv. 218, 1912) an account of experiments which demonstrate in a striking manner the importance of the pancreatic function. The heart of an animal made diabetic by removal of the pancreas is shown to leave unchanged any sugar supplied to it by way of the circulation, while under similar experimental conditions the heart of a normal animal uses the supply. When, however, a pancreatic extract is added to the blood, the heart from the diabetic animal also utilises the sugar. Such experiments show clearly that the pancreas influences the processes of utilisation and is not concerned merely with the maintenance of stability in carbohydrate deposits. They do not decide, however, whether oxidation is directly accelerated or whether the pancreas promotes a process which necessarily precedes oxidation.

body possesses means of dealing with other simple carbohydrate molecules. Hexose sugars isomeric with dextrose can suffer metabolism. Fructose, mannose and galactose, for example, are broken down in the body and, as a preliminary to further change, can be converted into glycogen, the first-named sugar almost as readily as dextrose itself. Now, the glycogen molecule is an aggregate of a number of dextrose molecules and would appear to be always the same substance, whatever simple sugar has acted as its precursor. The physiological occurrence of such a moulding of sugar molecules as this betokens raises chemical considerations of no small interest. The biochemist would miss his vocation if he were content in such cases to resort to the magic of bioplasm as a sufficient explanation. A description of the actual happenings in the definite terms of chemical dynamics is his ultimate and perfectly reasonable aim. To say merely that these sugars are "assimilable" and therefore can be metabolised is to take an attitude towards the operation of the bioplasm such as spectators take towards those of the conjuror when he puts a golf-ball under a hat and later displays a rabbit in its place. The physiological conversion of one of the hexose sugars into another is comparatively easy to understand now that it has been shown that dextrose, fructose and mannose are mutually interconvertible in alkaline aqueous solution. Starting with a solution of any one of them, we find that after a time it contains all three in equilibrium. By a process which ultimately involves an intramolecular shifting of hydrogen atoms, though it is probably in essence one of alternate hydration and dehydration, any one of these sugars can assume an "enolic" or unsaturated form. This form is the same in the case of all three sugars and from it all three may be produced. The relationship will become clear when the formulae of these carbohydrates are considered :



The temperature and alkalinity of the body are not such as would induce these changes with the required velocity and it

is probable at least that they are determined by specific enzymes. To some upset in the normal equilibrium of such enzymic activity may be ascribed the fact that on rare occasions fructose is excreted by individuals even when their diet contains no carbohydrate which could yield fructose during digestion. In such cases the normal direction of isomeric change would appear to have suffered reversal, fructose being formed from dextrose. For it is usually and very justifiably assumed that normally, while dextrose is directly taken up by a cell, the isomeric sugars are converted into dextrose before the metabolic grip takes hold upon them. A certain speculation, however, with regard to this matter may be excused. Yeasts can ferment any of the above three sugars with approximately equal ease and the fermentative breakdown in each case is on precisely similar lines. It has been suggested that the reason for this is that the fermentation starts with identical material in each case—namely, the enol of the sugars produced by a preliminary enzymic change.¹ If this suggestion have any weight in connexion with fermentation, it is justifiable to apply it to the animal cell; and if dextrose itself must be enolised before the cell can condense it to glycogen or impress other changes upon it, it is clearly possible that the metabolic failures responsible for glycosuria may include a failure to enolise. The conversion of any one of these related sugars to the enol form may perhaps be conditioned by a distinct enzyme, so the interesting but very obscure circumstance that diabetics can often utilise fructose when their power to utilise an equal quantity of dextrose is lost lends some support to the above conception. It must be admitted, however, that it is essentially speculative.

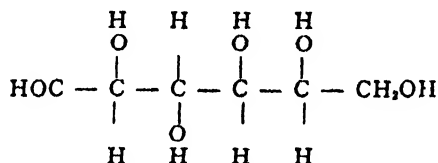
We shall in any case clearly gain light upon the normal metabolism of sugar if we can decide what precisely is absent when, in conditions of clinical or experimental diabetes, the body fails to oxidise that substance. The deficiency is by no means the same in all varieties of diabetes or glycosuria and the hope is reasonable that by the time we have classified these varieties we shall know something of more than one of the links in the chain of normal metabolic change. A fact of great significance is that in spite of the failure to oxidise sugar, the diabetic organism shows no failure in general oxidation power.

¹ Cf. E. F. Armstrong, *The Simple Carbohydrates and the Glucosides* (Longmans, 1910), pp. 52 *et seq.*

During long periods, in spite of the escape of sugar from the body, life in the diabetic is continued with combustion processes in full vigour. This is an aspect of affairs which lent a certain strength to Pavy's position. He writes scornfully of those who speak as though diabetes were due to sugar failing to be burnt in the system: "Nothing can be more gratuitous, unfounded and misleading. There is not a particle of evidence to show that defective oxidising power exists in connexion with diabetes. The real fault is a condition antecedent to the oxidising operation."

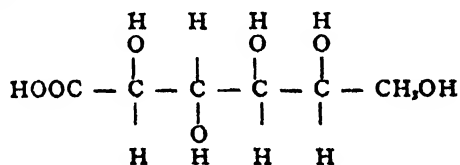
There are, indeed, many facts to suggest that sugar, when normally burnt, is not burnt as sugar but that its oxidation follows some previous change.

Consider the constitution of the sugar molecule:



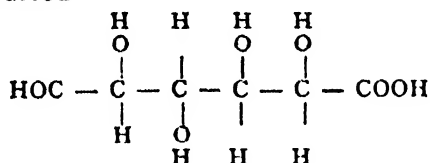
If we were to assume that the free-molecule suffers oxidation in the body and were to try to decide *a priori* the probable steps involved in its oxidation, chemical and physiological considerations would alike suggest the easily oxidisable aldehyde group ($-\text{COH}$) as the first point for oxidative attack. A deficiency in the diabetic might then be the absence of a mechanism for oxidising this aldehyde group. Experimentally, indeed, it has been found that if this group in sugar be oxidised to a carboxyl ($-\text{COOH}$) group before it is administered to a diabetic, then complete oxidation follows.

Gluconic acid—

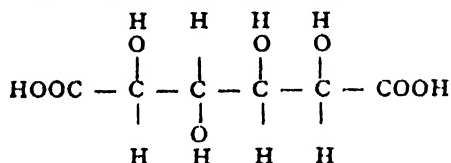


—is completely oxidised when the oxidation of sugar fails. But it is no specific failure to deal with an aldehyde group that stamps the diabetic, as he can equally well oxidise the substance glycuronic acid, another primary oxidation product of sugar in

which, however, the aldehyde group is intact and the group ($-\text{CH}_2\text{OH}$) oxidised—



So too, he can oxidise saccharic acid—



—from which both these groups are absent. Of these closely related substances only sugar itself is not oxidised.

These facts are in any case puzzling; but with other facts they make for the belief that what is absent in diabetes is not an oxidative mechanism but a means to carry out a process which, in the case of sugar, normally precedes oxidation in the tissues. This process might be either a non-oxidative rupture of the free molecule of sugar or it might be an event which occurs while sugar is part of a complex.

In connexion with the former possibility certain experimental work has been supposed to show that animal cells deal with the sugar in the way that the yeast-cell deals with it—that the primary change is alcoholic fermentation and that what is submitted to actual oxidation is the alcohol. More recent and more critical experimental studies greatly diminish the probability of this rather startling suggestion. But we are left with more solid ground for a belief in another form of cleavage or rather for a cleavage stopping short at what is possibly the precursor of alcohol in yeast fermentations. It is certain that lactic acid is formed in animal tissues and there is a strong probability that it is formed from carbohydrate. What evidence we have concerning the significance of its appearance is almost entirely derived from a study of muscle metabolism.

In muscles lactic acid makes its appearance in appreciable quantity only when the supply of oxygen is relatively deficient. When such deficiency exists the acid appears in the muscles of the living animal and is then, to some extent, excreted

in the urine. The amount increases with the activity of the muscles, the maximum being observed when strenuous muscular work is done under conditions which interfere with normal aeration through the lungs. Exertion at high altitudes, where the oxygen tension of the atmosphere is low, has been shown, for instance, to lead to an increase of lactic acid in the blood. More precise information with regard to its significance has been obtained by studying the processes which occur in excised but still surviving muscles, especially in the organs of cold-blooded animals, such as the frog, in which chemical changes are slow and more easily analysed. In these, the formation of lactic acid has been shown to be related to the processes of surviving life. It ceases at a time when the muscles no longer contract upon stimulation, so that the production of the acid cannot be classed with post-mortem changes. If the quiescent muscles are well supplied with oxygen, lactic acid at no time appears in them in appreciable quantity; but if oxygen be available it accumulates steadily up to the point of death. Now an excised muscle can contract vigorously during a considerable period in the complete absence of an oxygen supply. What then is the source of energy under these conditions? Since carbonic acid is given off in the absence of a contemporary oxygen supply, a belief, shared by Pavy, that the living tissues contain "intramolecular oxygen" has long been held. Oxygen, it is thought, is "built up" into the bioplasmic complex along with oxidisable material. When energy is to be liberated there is a change within the complex from less stable to more stable configurations and oxidation products, especially carbon dioxide, are produced. Recent critical experimental work has, in my opinion, deprived this belief in intramolecular oxygen of all foundation and I believe that its disappearance will mark an advance in our understanding of living processes. The carbon dioxide given off by a tissue when deprived of oxygen is liberated from the alkaline carbonates, always present in tissues, as the result of the accumulation of organic acids, of which lactic acid is certainly the chief. Such carbon dioxide therefore has no direct metabolic significance whatever.

A very instructive observation has shown that when a muscle is made to pass from a quiescent condition to one of active contraction in the absence of oxygen, there is no increase

in the evolution of carbon dioxide at all proportionate to the work done. No acceleration in its evolution is observed beyond what is accounted for on the lines just mentioned. But lactic acid does increase as a result of the contractions and increases at a rate proportionate to the work done. Its production is undoubtedly due to the processes which yield energy to the contracting muscle. Now if an excised muscle which has accumulated lactic acid as a result of oxygen deficiency be given a supply of oxygen, its lactic acid proceeds to disappear and if, as I have already stated, the oxygen supply be adequate from the first the acid never accumulates, there being proportionality between its formation and removal. Upon such facts as these is based what I believe to be the sound view that the energy of muscular activity is derived from a non-oxidative molecular breakdown of which lactic acid is a product. Upon this breakdown follows an oxidative removal of the products which normally keeps pace with their production. A careful study of the thermal relations of the phenomena has largely justified this view.

Now if we were quite sure that the lactic acid which appears in muscle were derived from sugar directly, we should have clear evidence for the occurrence of that change in the sugar molecule, preceding oxidation, which we were seeking in order to explain the existence of a normal oxidative power in the diabetic organisation side by side with its inability to oxidise sugar. There is every probability that the lactic acid is derived in some way from carbohydrate but the facts prevent our taking a quite simple view of the relation. The derivation of lactic acid from dextrose involves only a rearrangement of atoms in the sugar molecule— $C_6H_{12}O_6 = 2C_3H_6O_3$ —and the change leads to a very small liberation of energy, some 3 per cent. only of the total energy in the sugar being involved. A calculation of the actual quantities concerned, however, has led to the belief that the energy so liberated is, as a matter of fact, sufficient to supply the contracting muscle with its requirements; but a very recent investigation into the heat production of muscle during survival life points to the fact that the actual precursor of the lactic acid must possess at least 10 per cent. more energy than the acid itself¹; dextrose, as we have seen, contains only some 3 per cent. more.

¹ A. V. Hill : *private communication*.

stages, which, however rapid, are isolated in time and, may be, in place. One organ, we believe, may deal with some of the stages and quite another organ with the later ones.

In connexion with the conversion of sugar into fat, attention has recently become fixed upon the possibility that so simply constituted a substance as acetic-aldehyde (CH_3COH) is formed upon the way. The aldehyde is formed by the partial oxidation of dextrose or of its derivative, lactic acid, and may be supposed to undergo condensation and to give rise to fatty acids. There are suggestive, if not conclusive, experimental results in support of this view; it is also in accordance with the familiar but no less remarkable fact that physiological fatty acids contain always an even number of carbon atoms in their molecules. This would clearly be the case if condensation of a two-carbon aldehyde were responsible for their formation.

FORMATION OF SUGAR FROM PROTEIN

That sugar takes origin from protein in the body is shown by the quantitative study of certain physiological phenomena, especially as they occur in carnivora. The fact is abundantly evident in the phenomena of diabetes. In that condition as experimentally induced and in the severer cases of the disease in man, over half of the total energy contained in the protein of the food appears in the excreted sugar. Whether this should be taken as showing that so large a proportion as this normally assumes the form of sugar, the diabetic error merely bringing the sugar into view; or whether an abnormal breakdown is involved in diabetes we cannot yet decide but from general physiological considerations the former possibility is the more likely.

The chemistry of the transformation is, perhaps, on the whole, more easy to understand than that of sugar into fat, though as little decided by experiment. The administration of certain of the individual amino-acids which are contained in the protein molecule has been shown to increase the sugar output in diabetes; and the whole mixture of them, as obtained after hydrolysis of protein, yields as much sugar when administered to a diabetic dog as does an equivalent weight of the intact protein. Many are now working at this type of problem and there is no reason why we should not arrive at a knowledge of

the detailed steps in the transformation of amino-acids into sugar.

Sugar, then, can be excreted in diabetes in large quantity, when carbohydrate, as such, is completely absent from the diet and even when there is no longer a store of glycogen in any of the tissues. I find it very difficult to appraise exactly the attitude of Pavy's mind towards these facts, which can scarcely be reconciled with his view that purely assimilative errors so predominate in the picture of diabetes. The patient, it is true, in a great number of cases of the clinical disease, ceases, or nearly ceases, to excrete sugar, when carbohydrate is, as far as possible, removed from his diet, a method of treatment closely associated with Pavy's name. In such cases it is easy to believe that the error is solely on the side of assimilation. But Pavy was from the first, of course, familiar with the severer forms of the disease in which sugar continues to appear whatever the dietary. Until the quantitative work of recent years had been done there was no definite proof that, even in these cases, the sugar arises directly from protein and Pavy seems to have been slow to admit that there was any but an assimilative error even in the severest cases. Eventually he writes of a "faulty tissue-breakdown"; but seems, in some way, to reconcile the facts with his fundamental view, by assuming that the circulation of unassimilated sugar, which alone is present in the earlier stages of the disease, is the actual cause of the disordered katabolism of protein which may be established later.¹ When, in a discussion concerning normal phenomena, he deals with the proof that protein can yield sugar in the body in so large a quantity, he merely uses it to support the view that sugar is incorporated into protein during intestinal assimilation.²

A noteworthy circumstance characteristic of the diabetic condition is that, though the tissues are bathed with a solution of sugar stronger than that to which they are accustomed (for in all forms of diabetes save that due to phloridzin, which is dealt with later on, there is excess of sugar in the blood), there is no inhibition of sugar-producing processes. One would suppose, from considerations of chemical equilibrium, that these processes would be automatically slowed. On purely teleological grounds and looking at the matter from the side of

¹ *Carbohydrate Metabolism and Diabetes*, pp. 114, 115 (1906).

² *Ibid.* pp. 50-51.

utilisation only, we can perhaps understand that if a process which necessarily precedes utilisation (*supra*) be slowed by a deficiency in the chemical mechanism, an effort to increase its velocity by increasing the concentration round the cell would follow. Von Noorden speaks of the cells in diabetes as continuously feeling the need of sugar, though surrounded by the ample supply which they are unable to use. They still send out, therefore, those normal chemical stimuli which lead to the mobilisation of sugar and the supply continues in spite of the failure to utilise it. Pavy rejected this conception of a "call" made by sugar-hungry cells. So long indeed as diabetes involves only a failure in assimilation of the carbohydrate eaten, there is no need for any such assumption and, in any case, it is not a very satisfactory one. But when a large production of sugar from protein is established and continues, in spite of the excess of sugar circulating, some explanation of the fact seems called for.

Von Noorden's assumption, in so far as it involves a paradoxical "call" for sugar when so much is available, is perhaps unnecessary. We have seen that the diabetic organism liberates approximately as much energy under given conditions as does the normal organism under similar conditions. As this energy in the case of the former is obtained to a very much smaller extent from carbohydrate, it must be got from proteins and fat. If now it be a normal thing, as most assume and as Von Noorden assumes, for a certain fraction of the protein molecule to pass through the stage of sugar during its breakdown in the body, then that fraction is unavailable for the diabetic animal, which in so far as it makes use of protein to yield energy must rely upon the residuum of the molecule which does not pass through the sugar stage. But on the above assumption, the breakdown which yields this residuum must also yield sugar, even if it occur on perfectly normal lines. The call of the tissues, therefore, is not for sugar but for energy and the continued mobilisation of sugar is a secondary phenomenon. Nevertheless, researches carried out during the last decade have led to a belief on the part of many that neither inability, however caused, of the liver to function in regulating the rise and fall of glycogen, nor inability on the part of the tissues to utilise the sugar brought to them, nor even a combination of these failures will account for all the phenomena of diabetes, at any rate, as it

is observed in its severest form in man. The view is being forcibly expressed that the production of sugar in the body is an independent variable, determined, it may be, by the activity of specialised organs.

TEMPORARY GLYCOSURIA. THE LATEST THEORIES OF DIABETES

No one who has any acquaintance with metabolism can doubt that the normal utilisation of sugar is a process of a nicely balanced nature which an extraordinary number and variety of events can upset. Even the normal man has his limit of tolerance for sugar and the degree of tolerance can be easily modified. Temporary glycosuria may appear during many departures from health which have nothing to do with diabetes. It is induced by psychic strain or shock, by critical physiological events, such as pregnancy, even by sudden exposure to cold. It often follows as a secondary effect from the action of certain drugs on the body. But a condition of glycosuria which has received special attention of late is that associated, not with the absence or depression of an organic function but rather with the hyper-functioning of certain organs; in particular, of the thyroid, adrenal and pituitary bodies. I must not stop here to consider the evidence for this association. I can only point out that the facts have led to the conception that the normal equilibrium of carbohydrate metabolism involves a balance between factors, such as the activity of the glands just mentioned, which are concerned with the "mobilisation" of sugar and factors, such as the pancreatic function, which are concerned in its utilisation. The balance may be upset from either of two sides. The mobilisation of the sugar may be too rapid for normal utilisation processes to deal with it or the power to utilise it may diminish and so fail to cope with the normal supply. In either case, glycosuria results and may be intensified in certain cases, when there is at once over-production and under-utilisation.

This conception, for which the school of Von Noorden is chiefly responsible, is at once the latest addition to our views upon normal carbohydrate metabolism and the basis of the most recent theory of diabetes.

It is one of great interest but it cannot be said to be upon a firm foundation yet. Although it is beyond question that the internal secretions of the ductless glands just mentioned affect

the adjustments of metabolism, it is by no means certain that they influence the equilibrium of carbohydrate so greatly as the theory demands. The discussion, at any rate, has taken us away from the teachings of Pavy, who had but little opportunity of appraising so recent a view.

There is a form of glycosuria, experimentally induced, which has been much made use of in laboratory studies but which differs in a fundamental aspect from the vast majority of cases of spontaneous diabetes. I must refer to it before closing because it occupied Pavy's attention and lent some support to his views. When the substance phloridzin, a crystalline glucoside, is administered to animals, intense glycosuria is induced. The great difference between this and other forms of diabetes is in the amount of sugar in the blood, which becomes less than normal under the influence of the drug, instead of greater. Although there is still obscurity with regard to the exact mechanism of the action of this drug, there is no doubt that its seat is in the kidney. The view with regard to phloridzin diabetes which has been generally accepted is that of Von Mering, who discovered the phenomenon. He held that the effect of the drug is to increase the permeability of the kidneys for sugar. This leads to a lowering of concentration in the blood and a liberation of sugar from the organs to restore the deficiency. So long as the drug is in action, this process is continuous and leads to a large excretion of sugar.

It is clear, from what has gone before, that such a view would not square with Pavy's fundamental conception. In 1903, in conjunction with Brodie and Siau, he published some very interesting experiments which showed that a kidney removed from the body and perfused with blood containing phloridzin could excrete a quasi-urinary fluid containing more sugar than was lost by the blood perfused. Other experiments showed that if all the abdominal viscera were removed from an anaesthetised animal with the exception of the kidneys, the injection of phloridzin still produces a notable excretion of sugar.

Altogether these experiments seem to establish the fact that sugar is formed in the kidney itself and Pavy's view was that under the influence of the drug the renal cells acquire a power of splitting off sugar from some complex in the blood. Certainly these experiments offer the best evidence available for the circulation of sugar in some definite combination.

PAVY'S VIEWS TOO LIMITED BUT HIS TEACHING STILL
SUGGESTIVE

In dealing with Pavy's teaching I have found it necessary to point out that in some fundamentals it is incompatible, not only with the views of the majority (which would be a small matter) but also, as I believe, with physiological probabilities. But I shall have given a wrong impression, however, if it be concluded that Pavy held views devoid of basis or that what was special in his teaching is now without significance. There remains indeed much that should yet stimulate experimental research; it may even be said that quite the most recent experiments have given results which, in a sense, support his special views.

The primary arrest of the sugar which leaves the intestine is a process that is still not quite clear to us. Physiologists in placing the seat of arrest wholly in the liver are faced with the remarkable experimental fact that the establishment, in the dog, of Eck's fistula, a proceeding which permits the blood flowing from the intestine to enter the general circulation without passing through the liver, is not followed by glycosuria, even when the animal is digesting starch in abundance. One cannot but feel, even if it be impossible to accept Pavy's theory of local assimilation by the intestinal leucocytes, that such facts warrant a further inquiry into the functions of the gut in carbohydrate metabolism. As regards the form in which sugar is carried by the blood, it seems clear that the greater portion of it, if in any combination at all, is so loosely held as to be liberated when the blood proteins are coagulated. The latest observations agree, however, with those of Pavy, in showing that some more complex carbohydrate also exists and there is little doubt that the further study of this question, which he was planning at the time of his death, would have been of great value. We certainly do not yet possess full information either as to the transport of carbohydrate or as to the significance of that part which circulates in what Pavy called the "amylose" form.

Pavy, when looking for those errors which lead to diabetes, sought them, as we have seen, almost exclusively on the assimilation or constructive side of metabolism. His views were, we may say, too limited in this respect; but quite recent research seems to show that, during the last decade, too little

attention has been given to the possibility of assimilative errors. We have seen that there are now experimental grounds for believing that the influence of the pancreas in carbohydrate metabolism is exerted in connexion with some synthesis (of which the formation of glycogen is possibly the first stage) which precedes the final destruction of sugar in the body; and quite the last word upon metabolism, which Pavy would have been pleased to hear, suggests that "carbohydrate in some form or other is absolutely essential for the synthesis of protein within the tissues."¹ Clearly we have not yet the knowledge to appraise fully Pavy's views or any other views of a dogmatic sort, concerning the metabolism of sugar or the significance of diabetes.

It may shock many who are unfamiliar with the recent literature of physiology and pathology to learn that so few statements of a definite sort can be made with regard to so fundamental a matter as the fate of a basal foodstuff in the body. It may chill the heart of those who, at the beginning of their career, think of working at such problems, to view what seems the small harvest of Pavy's fifty years of labour. But Pavy had no sense of failure and none should be felt by those who have shared with him the attack upon these problems. If in this slight review speculations rather than facts have been prominent it is because, under the influence of Pavy, we have been considering the most intimate side of metabolism. This, from its very nature, is a region where experimentation is extraordinarily difficult and only recently has any serious attempt to explore it been made. Twenty-five years ago our equipment for the venture was most inadequate and the time has been mainly spent in preparation. Any one who will consider how much better we are equipped now will admit that the years have been well spent.

Owing to the labours of Emil Fischer and others, our knowledge of the pure chemistry of the simpler carbohydrates is both extensive and precise, so that when we seek evidence as to the course of molecular changes in the cell the possibilities and probabilities are for the most part clearly before us. But more than this: the moment the significance of intracellular enzymes became manifest biochemists made haste to

¹ Cathcart, *The Physiology of Protein Metabolism* (Longmans, 1912), p. 120.

put our knowledge of the dynamics of enzymic action upon a quantitative basis and have obtained information of the greatest value for studies upon the living cell. In the case of interactions which concern carbohydrates very important work has been done, in this country especially, by Croft Hill and by H. E. and E. F. Armstrong. We now know a great deal about the course of such interactions when conditioned by enzymes and such knowledge will make the experimental attack upon the central problems of metabolism infinitely more profitable.

The present moment is marked by a revival of interest in biological matters on the part of those who have high chemical qualifications and this is an auspicious circumstance. It must not, of course, be forgotten that results obtained in the chemical laboratory only become biologically valid when they have been checked in the animal and that our problems require the organised efforts of many workers with diverse qualifications. Because of the difficulties inherent in the complex conditions presented by our special material, the problems call continually for courage and patience—the courage and patience which characterised the subject of this memoir, F. W. Pavy.

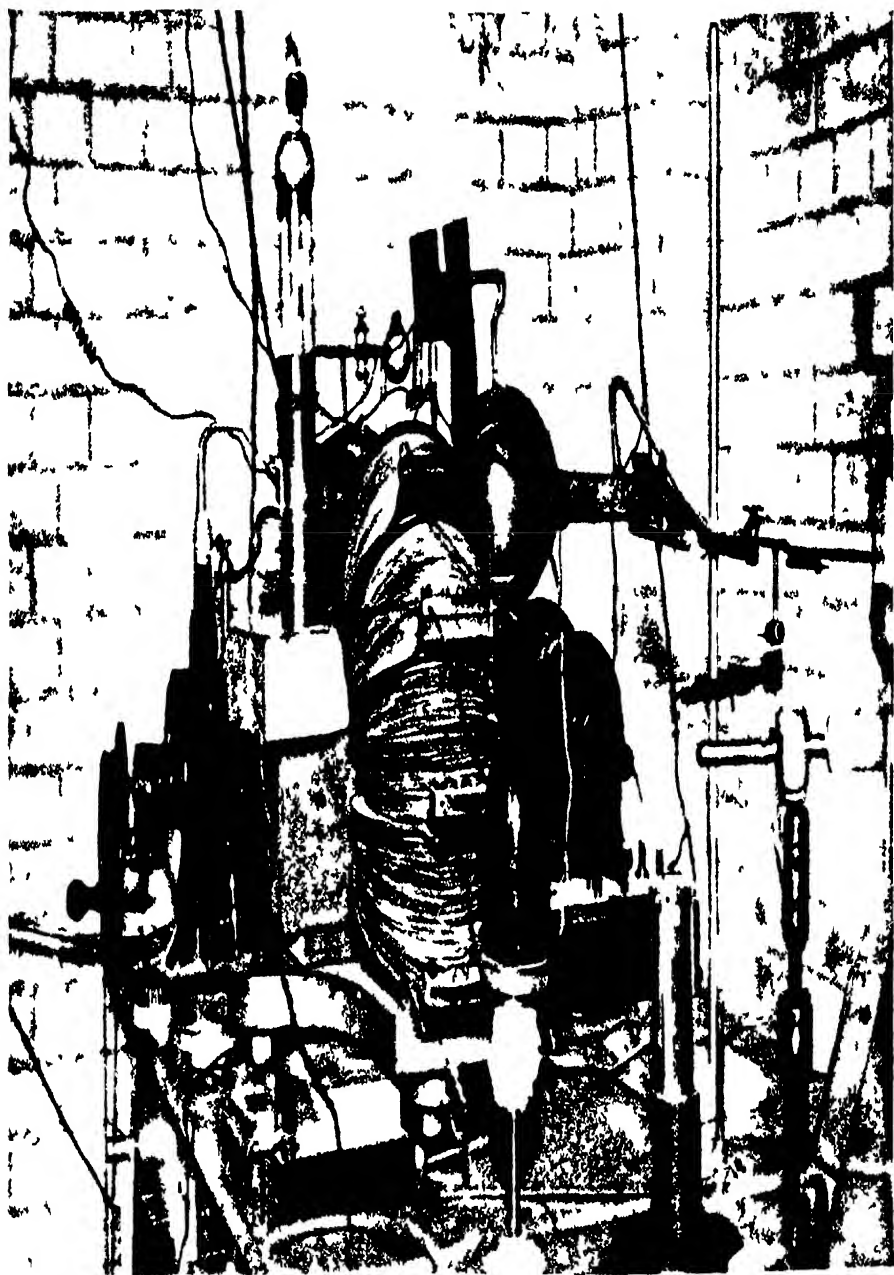
SIR J. J. THOMSON'S NEW METHOD OF CHEMICAL ANALYSIS

By F. W. ASTON, B.A., B.Sc., A.I.C.

No observer of the progress of "Molecular" Physics and Chemistry during the past decade or so can fail to have been struck by the extraordinarily intimate knowledge we have acquired, especially recently, of Atoms and Molecules—the individual units of complex matter. The results serve to confirm the shrewd estimate made by a great scientific thinker like the late Lord Kelvin that molecules are indeed almost inconceivably small compared with the masses of matter affecting our senses in everyday life. Thus the consensus of a variety of methods shows that a thimbleful of the air we breathe contains about a thousand million million million molecules, the average diameter of each of these being one hundred-millionth of an inch; or to give a more practical illustration, a molecule of carbon in the paper upon which this article is printed subtends to the reader's eye the same angle as would a normal human being at the distance of the moon.

To hope that an effect appreciable to our senses could be produced by a body so minute as a molecule would therefore at first sight seem absurd, yet this has been done in several notable instances in a most convincing manner. Thus in the spinthariscopes of Sir William Crookes we actually see the flash of light caused by the impact of a single α ray (which is a charged molecule of helium) upon a screen of zinc blende. Rutherford and Geiger have shown the measurable "kick" of a delicate electrometer due to the ionisation produced by a similar α ray. Whilst C. T. R. Wilson, with the aid of an apparatus recently exhibited at the Royal Society, has been able, in the most beautiful manner possible, both to see and to photograph the track of a single charged molecule.

The explanation of such large effects as these lies in the fact that the charged molecule constituting an α ray is moving at so prodigious a velocity that in its collision with other material



particles it is able to set free a quantity of energy out of all proportion to its mass; it is this Kinetic Energy or power of doing work, $\frac{mv^2}{2}$, which may be made appreciable by sufficiently increasing the velocity factor v , although the mass factor m may be inconceivably small. It is on this account that the Helium molecule of mass 6×10^{-24} of a gramme, when moving with a velocity 2×10^9 cm., i.e. about 100,000 miles per second, is capable of causing a flash of light appreciable to the eye when it strikes a fluorescent screen.

The novel and remarkable method of chemical analysis which is the subject of this article depends upon the fact that if we can communicate high enough velocities to molecules they will be able to produce appreciable and permanent effects when falling upon suitable material; also upon the fact that if such moving molecules can be electrically charged they become amenable to externally applied electric and magnetic forces and by their movements under these forces can be made, in a phrase, to *weigh themselves*. The method, indeed, is different from all other chemical methods of determining molecular mass, in that it deals with the individual molecule and not with large numbers.

It is the outcome of a long and exhaustive series of researches upon the nature of Positive Electricity which Professor Sir J. J. Thomson has been pursuing almost continually since he revolutionised modern views on electricity by his classical experiment with cathode rays, from which he inferred that negative electricity occurs as definite units—corpuscles or electrons—the mass of which is one eighteen-hundredth part of that of an atom of hydrogen. The principal field of these researches has lain in the so-called "Canalstrahlen" or Rays of Positive Electricity which Goldstein, as long ago as 1886, observed in a vacuum tube provided with a perforated cathode.

These rays were investigated afterwards by Wien, who showed that some of them at least carried a positive charge and had a mass of molecular order: it has, however, been the task of the head of the Cavendish Laboratory to explore, in a detailed and accurate manner, this wide and complex field of research. The subject of the present article is but a single offshoot of the work. It will be of interest to those who are unable to follow the original papers on the subject to know the method by which it has been demonstrated that just as light

from a flame can be split up by a prism into a spectrum showing the chemical constitution of that flame, so positive rays emerging from a perforated cathode can be resolved, in like manner, so that the several constituents of the gas in the discharge tube become obvious.

In order to apply the method to a gas, its particles undergo the following operations :

- (1) they are given a definite charge of electricity ;
- (2) they have a high velocity impressed upon them in a definite direction ;
- (3) they are allowed to pass through an electric and a magnetic field ;
- (4) finally they fall upon a fluorescent screen, a photographic

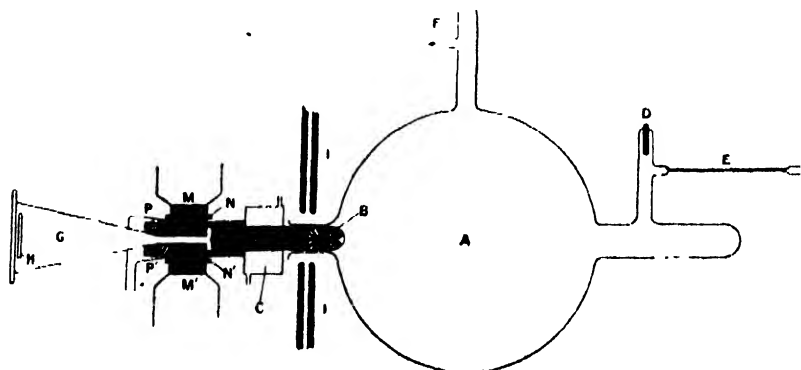


FIG. 1.

plate or some other suitable arrangement capable of recording the exact positions of the impacts.

Fortunately the first two conditions are fulfilled at the same time and automatically by submitting the gas to a high-tension electric discharge at low pressure. The gas is "ionised" by this treatment and the positive ions are projected with prodigious velocity towards the cathode ; if this be pierced with a small hole, so as to allow of their free passage, they will emerge on the other side as a stream of positively charged particles which may then be acted upon by the analysing fields.

It will be as well now to describe the particular form of apparatus which has been found to give the most satisfactory results. The main features are shown in the accompanying diagram (fig. 1). The discharge tube *A*, which is very similar

to an ordinary X-ray bulb, is a large spherical flask about $1\frac{1}{2}$ litres in capacity. Pushed into the neck of the flask and closely fitting it is the cathode *B*: this is made of aluminium and is so shaped that it presents to the bulb a hemispherical front provided in the centre with a funnel-shaped depression. The long, fine "canal-ray" tube extends from the bottom of this depression. If carefully centred and fixed so that its hemispherical head just projects into the bulb, this type of cathode gives a very intense beam of positive rays down its axis, *i.e.* into the "canal-ray" tube. The latter has been made in several different ways: as the accuracy of the method depends on the fineness of the emergent beam, it is essential that the tube should be perfectly straight and extremely fine. The best results have been obtained with brass or copper tubes drawn down until their internal diameter was of the order of 0.1 mm. The fine tubes are most carefully straightened—tested by sighting a bright light through them—and mounted in a thick soft-iron tube (shown black in the diagram), which not only protects them from injury but also effectually shields the rays passing through them from external magnetic fields; the latter is a very important point, as in so narrow and long a barrel—80 mm. is a convenient length—the smallest magnetic deflection would be sufficient to drive the particles against the walls of the tube and so prevent them from emerging. The cathode is kept cool during the discharge by means of a small water-jacket *C*.

The anode of the discharge bulb is an aluminium rod *D*, which is generally placed for convenience in a side tube. In order to ensure the gas under examination being as nearly pure as possible and also to keep its pressure constant, a steady stream of the gas is allowed to leak through an exceedingly fine glass capillary tube *E* and after circulating through the apparatus is pumped out at *F* by a Gaede rotating mercury pump. By varying the speed of the pump and the pressure in the gas-holder communicating with *E*, the pressure in the discharge tube may be varied at will and maintained at any desired value during considerable lengths of time. The pressure is usually adjusted so that the discharge potential corresponds to a spark-gap between brass balls 1–2 cm. apart in air, *i.e.* 30,000–50,000 volts. Positive ions, *i.e.* particles of gas carrying a positive charge of electricity, are formed in *A* by the discharge which is maintained by a large X-ray coil made by Cox. Under

the influence of the enormous electric field, they attain correspondingly high velocities and those which fall axially upon the cathode pass through the narrow "canal-ray" tube and emerge as a fine beam of "canal-rays."

The charged particles travelling in a definite direction, at a high velocity, are subjected to the analysing influence of electric and magnetic forces by causing the beam to pass between the pieces of soft iron PP' which are placed between the poles MM' of a powerful electromagnet. P and P' constitute the pole pieces of the magnet but are electrically insulated from it by thin sheets of mica NN' and so can be raised to any desired electrical potential difference by means of the leads shown in the figure. As the rays pass between the faces of PP' , they are subjected to the influence of electric and magnetic forces simultaneously and after they have been analysed, in a manner to be described later on, they enter the "camera" G and finally impinge upon the fluorescent screen or photographic plate H . In order that the stray magnetic field may not interfere with the main discharge in A , shields of soft iron, II , are interposed between the magnet and the bulb.

Fluorescent screens made of powdered Willemite were used in all the earlier experiments but as these only show the impact of the rays very faintly in a dark room and give no permanent record, they are unsuitable for the purpose of accurate measurements; a notable advance in technique was made by the use of photographic plates. When exposed to a beam of positive rays, the surface of such a plate undergoes a chemical change of a nature somewhat similar to that caused by actinic light and may be developed in the ordinary way, a more or less intense deposit of silver being formed wherever it has been struck by the rays. The plates which have been found to give the best results are the well-known Sovereign brand made by the Imperial Plate Co. The most convenient way of exposing the plate is to use a device which the writer has used previously in other experiments requiring accurate movement of an object in a high vacuum. It is roughly indicated in the accompanying figure (fig. 2), which shows the complete camera. The photographic plate is placed in a light frame supported by a silk thread; the frame can be wound up and down by means of a winch the axle of which works in an air-tight, ground joint. While the pressure, etc., is being adjusted, the plate is kept at

the top of a light-tight metal case and as soon as the fluorescent screen at *A* shows that the desired conditions have been obtained the plate is lowered into the field of the rays and a photograph taken. The exposures depend almost entirely on the diameter of the canal-ray tube and vary from three minutes to three hours. By the use of a long plate, as many as three photos could be taken before it was necessary to destroy the vacuum in the apparatus and introduce another plate. As it is usually desirable, for reasons which will be explained, to

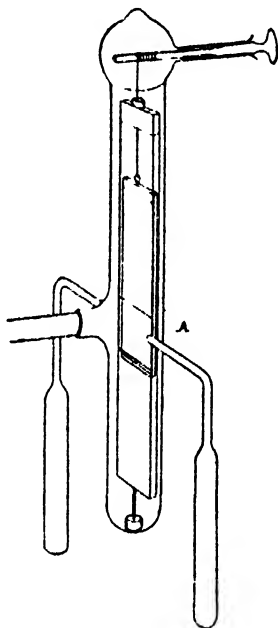


FIG. 2.

have as low a pressure as possible in the "camera," one or two Dewar charcoal tubes are attached to it and are immersed in liquid air while the photograph is being taken. As gas can only enter through the long and fine canal-ray tube the pressure in the camera may be very much lower than that in the bulb.

The illustration facing p. 48, which is from a flashlight photograph taken by Mr. Hayles, of the Cavendish Laboratory, conveys a good idea of the actual appearance of an apparatus set up by the writer with which a great many results were obtained. On the extreme right can be seen part of the gas

reservoir and just behind this the very fine capillary tube which allows the gas to leak slowly into the discharge bulb shown on the right of the large Du Bois electromagnet. In a corresponding position on the left of the latter is the "camera" made of glass tube partially covered with paper; this contains the plate-holder and supports at the top the glass "winch" by which the plate is raised or lowered. Behind the magnet may be seen the Gaede pump and the induction coil. Attached to the camera is the large Dewar charcoal bulb, which is cooled by immersion in the vessel of liquid air; the latter stands on the table, together with an accurate ammeter for measuring the current flowing through the magnet and a red photographic lamp for use during the removal of the plate when the exposure is ended.

The endeavour may now be made to explain, as briefly

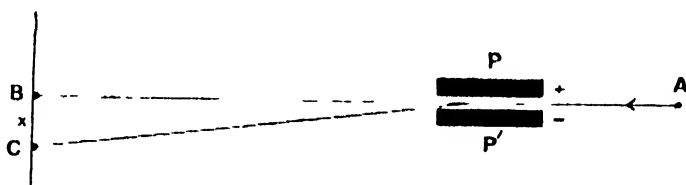


FIG. 3.

and simply as possible, how by subjecting the moving charged particle to an electric and a magnetic field, each at right angles to its path, both the velocity and the mass of the particle may be deduced.

Let A (fig. 3) be such a particle of mass m , carrying a positive charge of electricity e and moving with velocity v in the direction AB . If this particle be not influenced by electric or magnetic forces, it will obey the ordinary laws of motion and move in a straight line, striking a distant screen at a point B . If, however, we cause it to pass through an electric field of strength X between the plates PP' , it will be deflected away from the positive and towards the negative plate in the plane of the paper and finally strike the screen at some other point C , the displacement $BC = x$ being given by the equation :

$$x = k, \frac{Xe}{mv^2}$$

If now the electric field be cut off and PP' made the poles of a magnet of field strength H , the moving particle will be

deflected *at right angles to the plane of the paper* a distance y given by the equation :

$$y = k_2 \frac{He}{mv}$$

k_1, k_2 being constants depending solely on the dimensions and form of the apparatus used.

If a continuous stream of particles, all of the same mass, carrying the same charge (or what amounts to the same thing in this case, having the same *ratio* m/e of mass to charge) and moving with the same velocity, strike the screen shown in plan in fig. 4—which is covered with a layer of powdered Willemite, a substance that fluoresces strongly under the influence of the rays—a bright patch of light is produced at the point B , due to undeflected rays, when neither the potential

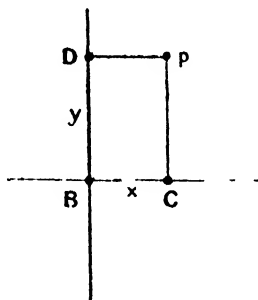


FIG. 4.

nor the magnet is on. The plates PP' being vertical, if the electric field only be on, the spot will be deflected to C ; if the magnetic field only be on, the spot will be deflected to D but if both are on together to a point p of which the horizontal and vertical displacements are x and y respectively. It is therefore only necessary to measure x and y and from the equations given above it follows that x is inversely proportional to the kinetic energy of the particle y and inversely proportional to its momentum y and that

y/x is a measure of the velocity of the particle ;

y^2/x „ „ „ $\frac{m}{e}$ or the ratio of mass to charge.

Now e can only exist as a multiple (and in general only a small multiple) of the charge on a single corpuscle and all the evidence up to now shows that this is invariable and

puscles are always to be found there, the result being that the behaviour of the positively charged particle is complicated in the following ways:

It may pick up a single negative charge and becoming neutral may pass the fields unaffected and strike the plate at the origin O , the "undeflected spot."

It may pick up yet another negative charge before emerging from the canal-ray tube and by retaining this throughout the fields may become a Negative Primary Ray and give rise to a parabola similar in all respects to the positive ones but in the opposite quadrant, as shown in fig. 5 by dotted lines.

It may be changed from a neutral to a charged particle of either sign or vice versa *during its passage through the fields*, thereby giving rise to rays which do not strictly obey the fundamental equations, as the values of X and H which affect them will not be constant but will depend on the position of their origin or destruction. These are called "Secondary Rays." The effect of these rays on the photograph or screen is exceedingly complex; indeed in the earlier experiments they completely overshadowed the genuine primary rays, so that it was only by designing apparatus in which the pressure in the camera could be kept low that the primary rays could be seen distinctly. Even with the apparatus in its present state, it is impossible to eliminate them entirely, especially when gases such as hydrogen or helium are present which are not completely absorbed by the cooled charcoal. Owing to the presence of secondary rays, the greatest care must be taken in interpreting the photographs, as the secondary rays may give parabolas which under certain conditions are quite indistinguishable from the true primary parabolas. Fortunately the relative positions of these false curves are usually changed when the photograph is repeated under slightly different experimental conditions. It is then possible to detect them, as no such change in the relative positions of the true primary parabolas is ever noticeable. The object in maintaining the lowest available pressure in the camera is to eliminate secondary rays as far as possible.

It will now be well to consider a few of the actual results in detail. The accompanying plates are reproductions from the original negatives and illustrate several typical cases. Plate I was obtained with nitrogen (made from air) in the tube, the

1



2



3



4



5



6



7



8



Reduced in reproduction to three quarters actual scale

magnetic deflection being small enough to show the two hydrogen lines. It will be seen that there are five very bright lines in each side of the magnetic zero; if the most deflected line be of mass unity, taking the squares of their relative deflections, the other lines correspond to masses approximately 2, 14, 28, 200. The five lines are evidently due to the hydrogen atom and molecule, to the nitrogen atom and molecule and to the mercury atom respectively, each presumably carrying a single charge.

All the parabolas end off approximately at the same distance from the vertical axis through the bright undeflected spot: that corresponding to the nitrogen atoms, however, has a distinct "beak" or feebler continuation which ends half as far away and therefore must be caused by particles having twice the kinetic energy of those causing the brighter part. It is quite impossible to suppose that these are due to nitrogen atoms which have fallen through twice the voltage, as the actual maximum voltage of the discharge tube never rose appreciably above that corresponding to the tips of the other parabolas; the most probable explanation is that the atoms of nitrogen forming the extension of the curve carried a double charge $+2e$ while coming up to the cathode and therefore reached it with twice the normal kinetic energy. If during their passage through the canal-ray tube they picked up a single negative charge $-e$, they would emerge as atoms with a single positive charge and so would fall upon the same parabola but at a distance half as far away from the magnetic axis. If this view be correct, we might expect some of these doubly charged atoms to retain both charges throughout the fields; they would then behave exactly as would particles of mass 7 with a single charge $+e$, as the position of the parabola depends only on the *ratio* m/e . On looking for such a parabola, it can be seen clearly between the nitrogen atom and the hydrogen molecule, though it is naturally rather faint. Similar evidence of doubly charged particles will be seen in several of the other plates.

Though the negative in Plate I is a good one to reproduce in print and to illustrate the general characteristics, it is by no means the best type for actual measurement, as the lines in it are much too thick and bright. It would be quite impossible to reproduce satisfactorily the plates with which the best metrical results have been obtained, as a line can be measured with great

accuracy if the canal-ray tube be sufficiently fine even when it is only just visible on the negative.

For measuring purposes the negative is clamped in a special apparatus and a needle, mounted on a slider so that its point just does not touch the gelatine, is moved across the parabolas in a direction parallel to the magnetic axis OY (fig. 5); whenever the needle lies exactly over a parabola, its position is read on a vernier scale. In the case of a fine line the position can be determined to about $\frac{1}{10}$ mm.

In order to give some idea of the measurements which can be made in this way, the actual records of an experiment with a very fine canal-ray tube working satisfactorily may be quoted.

Gas in discharge tube *air* at about $\frac{1}{100}$ mm. pressure. Potential on plates 166 volts. Current through magnet 2.00 amperes. Exposure $1\frac{1}{2}$ hours. Discharge potential equivalent to spark-gap $1\frac{1}{2}$ cm. in air; d is the displacement in mm. from electrical axis; m is the corresponding mass obtained from the inverse square of d expressed relatively to mercury as 200.

d .	m .	Probable cause of line.	
5.25	200	Hg +	Mercury atom with single charge.
7.40	100.2	Hg ++	Mercury atom with double charge.
9.15	64.6	Hg +++	Very faint line, possibly mercury with triple charge.
11.30	43.0	CO ₂ +	Very faint, probably CO ₂ .
14.05	28.0	N ₂ +	Nitrogen molecule (brightest line).
18.50	16.0	O +	Oxygen atom.
19.70	14.1	N +	Nitrogen atom.
21.50	11.9	C +	Carbon atom.
26.15	8.0	O ++	Oxygen atom with double charge.
28.1	6.98	N ++	Nitrogen atom with double charge.
30.35	5.98	C ++	Carbon atom with double charge.
52.2	2.02	H ₂ +	Hydrogen molecule.

(Carbon and its compounds were present as impurities derived from the apparatus; these can only be eliminated with great difficulty by prolonged washing with oxygen. Lines due to such impurities are, as a rule, very faint in comparison with those due to the gases known to be present in quantity.)

Here we have twelve distinct parabolas, not counting that due to the hydrogen atom which has been thrown off the plate by the large magnetic field. Of these all the bright ones fall exactly on positions expected from the gas that filled the tube, their masses agreeing with the generally accepted molecular and atomic weights to about 1 per cent.

A word of caution may well be given here in connexion with the relative photographic intensities of the lines. These are entirely misleading and incorrect, as one might very well expect on seeing that in Plate I hydrogen gives almost the brightest lines in a tube supposed to contain practically pure air. A trustworthy electrical method has been devised recently by which the true relative intensities of the lines can be deduced from the total charges carried by the particles which give rise to them; the results show that a hydrogen line appearing on the plate or screen as the brightest line of the set may really not be one hundredth part as intense as the lines corresponding to the gas with which the tube is filled.

From experiments already made by the electrical method, we may say that roughly speaking the true intensities of lines due to a given gas are proportional to the quantity of that gas present, whilst the photographic intensity of lines of equal true intensity is far greater in the case of those produced by particles of lighter mass.

To readers interested in chemistry a short description of the specific behaviour of a few individual elementary substances may be of interest. To begin with, it is a fact of the very first importance to the student of the nature of electricity that up to now, though every possible scrutiny has been applied, *no positive ray having a smaller mass than that associated with a hydrogen atom has been detected*. Elements of lower atomic weight, if present, make no appearance on the sensitised surfaces used to record the rays, neither does it seem possible for the hydrogen atom itself to carry more than one charge.

Hydrogen.—The lines due to H_{1+} and H_{2+} , largely no doubt owing to their very exceptional photographic efficiency, appear on practically every photograph taken of the part of the magnetic spectrum which includes them. They can be eliminated, however, by thoroughly rinsing out the tube with highly purified oxygen. Oddly enough, considering the chemical properties of the element, atomic hydrogen also appears repeatedly with a negative charge, the negative parabola due to the hydrogen atom being plainly visible in Plates I and II. If hydrogen be mixed with a small percentage of some other gas, such as nitrogen, a very remarkable line sometimes makes its appearance which corresponds to a hypothetical

substance H_{3+} . A photograph showing this line is reproduced in Plate II; though it is always faint when compared with H_1 and H_2 the parabola is nevertheless genuine and has been repeatedly obtained.

Oxygen.—This gas has probably been experimented with in a more nearly pure state than any other, as it combines with all the impurities given off by the apparatus forming compounds which can be removed by means of liquid air. Plate III was taken with this gas. H_1 and H_2 have practically disappeared and nearly the whole of the intensity is in the lines corresponding to +16 and +32, O and O_2 respectively. There is a very strong negative line O_- at -16. This O_- line appears on nearly all plates taken when oxygen is present, either free or in combination. No negative corresponding to O_2 has been detected in highly purified oxygen but the line sometimes appears when other gases are present. The very obvious extension of the O_+ line in Plate III indicates the tendency of the oxygen atom to take up a double charge.

Nitrogen appears as N_{++} , N_+ and N_{2+} ; it never gives a negative parabola. In some of the nitrogen photographs a faint line is found at 42-43 which Prof. Thomson thinks may be due to a compound N_3 or N_3H . If made from air, nitrogen shows the argon line corresponding to mass 40.

Carbon appears as C_{++} , C_+ and C_- when compounds such as the monoxide and dioxide are used. Plate IV, which represents carbon monoxide, shows the negative O and C lines quite clearly and also doubly charged positive ones. On using certain organic compounds, a negative parabola corresponding to a mass 24 is found, which seems to be due to a molecule consisting of two carbon atoms carrying a single negative charge.

Organic compounds give very complex results but it is beyond the scope of this article to discuss these. The case of methane, CH_4 , however, is comparatively simple and of particular interest. In the case of this gas, if a very narrow canal-ray tube be used, a group of five distinct parabolas is observed differing from each other by mass 1 and corresponding to C , CH , CH_2 , CH_3 and CH_4 respectively, each carrying a single positive charge.

Chlorine and the other Halogens can be used in the form of their compounds with hydrogen or carbon. They are principally of interest because, like oxygen, they give strong negative atomic parabolas.

Helium is associated with a single very strong line of mass 4 corresponding to He_+ . As this gas cannot be removed from the camera by the cooled charcoal the secondary effects are usually very strong. Plate V shows the two faint hydrogen lines and the bright helium line. This plate is an admirable illustration of the danger of secondary rays. The apparent parabola just inside the He parabola, which corresponds to a mass 5 and might easily suggest a compound HeH , is really not a primary at all. If the pressure in the camera be allowed to rise rather higher, the effect shown in Plate VI is produced, bright beams of secondaries of both signs being the only visible rays.

Mercury.—This element possesses quite peculiar interest in connexion with these results. Its presence in the discharge tube in small quantities is, of course, to be expected, as the apparatus is exhausted by a mercury pump. Should mercury not be required, it can be frozen out with liquid air; in general, however, its presence is an advantage, as the mercury line cannot possibly be mistaken and gives a very valuable standard for measurement. The presence of large quantities of certain gases, notably oxygen and the halogens, involves its complete disappearance. The behaviour of mercury is in two ways quite inexplicable: in the first place, although the heaviest of all the elements yet measured, its photographic efficiency seems almost as great as that of the extremely light elements; and what is still more unaccountable, its parabola invariably seems to extend almost to the very origin itself and would require at least three or four charges upon a single atom to account for its enormous kinetic energy in the manner already indicated. Nearly all the Plates here show its characteristic line quite distinctly but Plate VII gives the most striking idea of its beautiful parabolic form and remarkable appearance when the strength of the magnetic field is made extremely high; the electrical displacement due to a single charge can be distinguished as a bright "bead" a short distance along it; the head of the other line (CO_+) in the plate is in the same vertical line. This mercury line 200 is almost always accompanied by the double charged one corresponding to 100, which can be seen plainly in Plate IV. Mercury is unique in that it is the only metallic element, with the doubtful exception of potassium, which as yet has given definite proof of its existence in positive rays.

Plate VIII, which was obtained from a mixture of hydrogen

and oxygen under conditions of fairly high pressure in the camera, has been included in order to give the reader some idea of the extreme complexity of the secondary rays, which in this particular instance form a perfect network of lines, straight and curved. Out of five apparently distinct lines on the negative side, only two, the H_1 and O_1 lines, are genuine. For a detailed discussion as to the origin and behaviour of secondary rays, the reader is referred to Prof. Thomson's original papers on the subject which have been published from time to time in the *Philosophical Magazine*.

From these few illustrations and brief descriptions, ideas of the possibilities and limitations of the method will doubtless have already been formed. As to the latter, some are obvious, such as the fact that in order to apply the method to the determination of atomic weights the substance analysed must exist in a state of vapour and be able to support an electric discharge. There is, however, another more subtle disability which is also known to affect ordinary spectroscopic analyses of gases: this is that a substance may be present in quite large quantities and yet its characteristic lines may not be apparent. When it was stated that mercury was the only metal so far clearly identified, it must not be understood that it is from any lack of trying others. As soon as the method was found to afford results of reasonable accuracy, Sir J. J. Thomson endeavoured to apply it to settle the much vexed question of the atomic weight of nickel, the value generally accepted by chemists appearing incompatible with the results obtained by physicists on studying the characteristic radiation of the metal. But although nickel carbonyl was passed through the tube and nickel chloride was vaporised inside it, the plates obstinately refused to vouchsafe the least indication of a nickel parabola and results with potassium were very nearly as negative. It seems almost inconceivable that these elements cannot exist as ions in the discharge tube but it is quite possible that they are incapable of retaining their charge after reaching the cathode and so are not analysable by the method. Another less likely possibility which may shortly be tested is that the parabolas are there but are incapable of affecting the screen or the plate. From the point of view of accuracy, the limitations of the method are almost entirely those of apparatus, design and technique; it is therefore to be supposed that they will be removed as experience grows.

As regards the very special interest and possibilities of the method, in the first place the sharpness of the parabolas obtained, which appears to be only limited by the possible fineness of the canal-ray tube, is the first rigorous and direct proof of an article of scientific faith which has been accepted during many years past without hesitation, namely, that the individual molecules of any given substance all have identically the same mass.

The point which will probably appeal most strongly to the imaginative mind is that connected with the almost inconceivably short time necessary for a particle to exist in order to register its mass. For since a moderate velocity for the positive rays is 10^8 cm. per second and 10 cm. is amply sufficient for them to gain their velocity and be deflected by the fields, compounds which have an existence of but the ten-millionth part of a second will infallibly be weighed on this impalpable balance. Hence it is that we need not be surprised at finding upon the plates lines corresponding to molecules found neither in the heavens above nor the earth beneath; nor need those of us who are chemists hold up our hands in horror at such unnatural and grotesque monsters of the world of molecules as H_2 , CH , CH_2 , CH_3 , N_2 , etc., etc. Rather should we look forward to this line of investigation as an extremely hopeful field in which to study the actual mechanism of dissociation, ionisation and chemical interaction. The method is applicable to the most microscopic quantities of a substance at disposal. That it has already yielded interesting results will, I hope, be apparent from this very brief account; there seems to be little reason to doubt that, as year by year the technique of the experiments is improved, results of equal and greater importance may be expected from it.

CONDITIONS OF CHEMICAL CHANGE

II. PHOTOCHEMICAL CHANGE IN GASES

PART II. EXPERIMENTAL¹

By D. L. CHAPMAN, M.A.

IN the preceding article, in which the views held by different investigators on the mechanism of the interaction of chlorine and hydrogen were explained and discussed more or less in detail, it was indicated that the most favoured hypothesis during several years preceding 1905 involved the assumption that an intermediate complex was formed, containing chlorine, hydrogen and water, of the type represented by the formula $[\text{Cl}_2]_x$, $[\text{H}_2\text{O}]_y$, $[\text{H}_2]_z$. It was supposed that this did not exist in a freshly prepared mixture of chlorine and hydrogen but was gradually produced when the mixture was exposed to light and subsequently destroyed so as to form hydrogen chloride and water, the production and decomposition taking place in accordance with the law of mass. According to this view, hydrogen chloride could not be generated in the system by the direct interaction of molecules of chlorine and hydrogen but only by the breaking-down of the unstable system in which they were associated with water. It was claimed that by this explanation it was possible to account satisfactorily for the accelerative influence of moisture on the change and also for the initial inert period which is generally observed when a mixture of freshly prepared chlorine and hydrogen is exposed to light. Moreover, the hypothesis supplied an intelligible account of another well-known peculiarity of a mixture of chlorine and hydrogen, namely, that when it has been exposed to light until the rate of formation of hydrogen chloride is constant (*i.e.* until the concentration of the complex attains its maximum value) and is then left in the dark during several hours, it regains in some

¹ A description and diagram of an actinometer with which most of the experiments on chlorine and hydrogen described in this article can be carried out are given at the end of the article.

measure its original property of temporary irresponsiveness to stimulation by light. The explanation of this phenomenon, which had received the name of "photochemical induction," was supplied by the assumption that the complex is slowly broken down in the dark into the substances from which it was built up—chlorine, water and hydrogen. The hypothesis in question was originally advanced by Mellor and afterwards modified by Bevan so as to account for the fact discovered by Draper and confirmed by Bevan that the inert period is of much shorter duration when the chlorine is exposed to light before being mixed with the hydrogen. The modification of the hypothesis necessitated by the confirmation of Draper's observation was obvious and simple. The complex must be formed in two stages: in the first, water molecules become united with chlorine molecules; in the second, the complexes thus produced become attached to molecules of hydrogen: the subsequent fate of the final product depending, as stated above, on whether it be permitted to break up in the light or in the dark. When examined in the light of the qualitative facts on which it was based, the hypothesis was not unconvincing. Bevan, moreover, maintained that, by means of experiments on the formation by expansion of clouds in moist chlorine and electrolytic gas, he had obtained evidence of the existence of peculiar compounds—presumably the postulated complex—which could act like gaseous ions as condensation nuclei for steam.

The view that hypotheses based on the assumption of the formation of intermediate complex compounds could adequately account for the cardinal or subsidiary phenomena observed in the study of the photochemical action of chlorine on hydrogen was contested by Burgess and the writer at the Cambridge meeting of the British Association in 1904 and also in the *Proceedings of the Chemical Society*, for two distinct reasons. The first objection was grounded on certain observations relating to the preliminary inert period recorded by Draper and confirmed by the authors. Under certain conditions when electrolytic gas was exposed to light, no hydrogen chloride was formed during a considerable period of time—sometimes in our experiments exceeding two hours—and then the rate of formation of the chloride rose in less than ten minutes to its maximum value. This result was contrary to the requirements of the intermediate complex hypotheses, according to any of

which the rate of diminution in volume of the electrolytic gas ought to have increased gradually and steadily from zero to a constant value. A second objection raised was that on repeating Bevan's experiments on the formation of clouds in the rapidly expanded gases, we failed to discover any facts which could not be readily explained without invoking the aid of a new class of condensation nuclei. Moreover Dyson and Harden (*Trans. Chem. Soc.* 1903, **83**, 29) had pointed out that the theory of an intermediate compound could not be reconciled with the "induction period" observed by them with an almost dry mixture of chlorine and carbon monoxide. It was evident that the hypothesis in question would have to be abandoned and that the true cause of the "period of chemical induction" had not as yet been disclosed. A systematic study of the conditions controlling the manifestation of the inert period was therefore undertaken in the hope of elucidating the cause of the phenomenon. It was soon discovered that the source of the initial inertness of the mixture resided as much in the liquid used in the actinometer to absorb the hydrogen chloride as in the gas itself; moreover, that the property of imparting inertness to the gas was possessed in very different degrees by different liquids. Experiments on the following lines had forced this conclusion upon us.

An actinometer, filled in the usual way with electrolytic gas, was exposed to light until the maximum rate of interaction had been reached; the duration of the induction period was duly recorded. The actinometer was then shaken so as to bring the liquid into intimate contact with the gas and again exposed to light; another inert period of shorter duration than the first was observed. On again shaking the actinometer and then re-exposing it to light, a third induction period shorter still than the second became manifest. By the constant repetition of these operations the contents of the actinometer were at length brought into such a condition that no further indication of an induction period was noticeable on shaking. The results show clearly enough that the cause of the inertness resides in the liquid and can be communicated to the gas and that it is destroyed on exposure of the latter to light. A series of experiments was next performed in which the absorbing liquids were aqueous solutions of salts and mineral acids. In these circumstances, the induction periods observed were often many times longer than when

distilled water had been used to dissolve the hydrogen chloride produced in the interaction. An aqueous solution of a particular specimen of crystallised barium chloride was found to be exceptionally effective in rendering chlorine inactive. Our attention was next turned towards the discovery of all the possible methods by which such aqueous solutions could be rendered incapable of imparting inertness to electrolytic gas. It was found that this could be best accomplished by the simple expedient of saturating the aqueous solution with chlorine and then boiling the solution until as much as possible of the chlorine had escaped. Attempts were next made to remove the residue of chlorine from a solution which had been treated in the above manner without at the same time introducing the obscure agent which is capable of rendering electrolytic gas temporarily insensitive to light, in order to provide ourselves with the means of testing conclusively whether the agent in question be a transitory and communicable property of the salt—some substance produced from the pure salt—or simply some foreign impurity contained in the original sample of the salt. One of the methods adopted for removing the last trace of chlorine from the boiled solution was to add a drop or two of a solution of potassium iodide and then to remove the liberated iodine with a solution of sodium thiosulphate; the solution from which the chlorine had been thus entirely removed was incapable of imparting inertness to electrolytic gas nor did it acquire the property on standing or on heating. We were thus driven to the conclusion that the inhibitive agent was neither an acquired property of the salt nor a substance developed from the pure salt but some unknown, widely distributed impurity contained in the original sample of the salt. Just as we were on the point of starting a series of experiments with the object of concentrating and isolating the impurity, a pure accident disclosed its identity. It was thought that a method better than that just described of removing the residue of chlorine from the boiled solution which had been saturated with chlorine would be to add just sufficient ammonia to destroy the chlorine; on introducing the liquid treated in this way into an actinometer which already contained a mixture of chlorine and hydrogen and then exposing the actinometer to light, an inert period of abnormally long duration was observed. The widely distributed impurity present in many

soluble crystalline salts, in mineral acids and in water, capable of imparting to a mixture of chlorine and hydrogen the property of temporary inertness towards light, was therefore very probably ammonia. It was then discovered that the solutions which previously had been found to induce the longest induction period were those which contained the greatest amount of ammonia; that ammonia-free water was incapable of rendering chlorine inert; moreover, that in relation to chlorine and electrolytic gas, a dilute solution of ammonia corresponded in every particular with the solution of crystallised barium chloride previously examined. Now ammonia itself cannot exist in the presence of chlorine, so that the actual substance which induces the inertness of the mixture must be some product of the interaction of chlorine and ammonia.¹ Nitrogen chloride would appear to be indicated as the cause by the following experiment. A dilute solution of ammonia was saturated with chlorine and divided into two portions; 5 cc. of the first portion were added to an actinometer containing chlorine and hydrogen, which was then exposed to light; the induction period was long. From the second portion the chlorine was removed by exhaustion and 5 cc. of the purified liquid was introduced into a similar actinometer; the induction period was very short. Hence it follows that the compound which is the immediate cause of the induction period belongs to the class of substances which are volatile and readily removable by exhaustion on account of their slight solubility in water. Moreover, it is destroyed by light and heat. Nitrogen chloride fulfils these conditions.

There was yet one outstanding fact which could not be reconciled easily with the view that nitrogen chloride was the sole cause of the induction period, the so-called phenomenon of "deduction." When an actinometer containing electrolytic gas and tap-water was exposed to light and shaken so as to destroy the whole of the nitrogen chloride both in the gas and in the liquid and the actinometer was left to stand in the dark during several hours, it was generally found that the gas had become inactive, *i.e.* that an initial inert period preceded steady combination on re-exposure of the insolation vessel to light. This behaviour could only be explained on the assumption that the water contained some nitrogenous organic substance

¹ *Manchester Memoirs*, Vol. XLIX. (1905), No. 13.

which was slowly decomposed at the ordinary temperature in the dark and gave rise to the formation of nitrogen chloride. In order to test this explanation an actinometer was constructed which could be charged and heated at a little over 100° . When the heating had been continued during about twelve hours the actinometer was allowed to cool and exposed to light. Interaction at once set in and even after keeping the actinometer in the dark during several weeks the photochemical change was not preceded by a preliminary inert period. The heating had destroyed the nitrogen chloride and other substances from which nitrogen chloride could be developed by the action of chlorine. It was subsequently found that inhibitors are formed slowly when chlorine acts on water containing albumen.

The so-called induction period is therefore caused by the presence in the gas of a powerful inhibitive impurity—nitrogen chloride—which must be almost completely removed from the gases before the chlorine and hydrogen can interact.

The facts detailed above were discovered and published early in 1905 in the *Proceedings of the Royal Society* and in the *Manchester Memoirs*.

A year later it was suggested by Luther and Goldberg¹—in a paper in which our work was mentioned but curiously enough not contested—that induction is essentially due to the contamination of the mixture of hydrogen and chlorine with oxygen. Any one who peruses the papers of Bunsen and Roscoe, Bevan, Mellor or those of the author and his collaborators will perceive that such a suggestion cannot possibly be entertained. Oxygen is not removed from a mixture of chlorine and hydrogen on exposure of the latter to light; if it be, the rate of removal is so slow that the effect of its disappearance cannot be detected by measurements of the velocity with which chlorine and hydrogen interact.²

The most remarkable feature of the inhibitory influence of nitrogen chloride is the enormous effect produced by an

¹ *Zeitschr. Phys. Chem.* 1906, 58, 43.

² I take this opportunity of proclaiming the untenability of Luther and Goldberg's suggestion, since the views of these authors on this question have been accorded a prominent place in the new edition of Nernst's *Theoretical Chemistry* and may through that source find their way into other text-books dealing with the subject of photochemistry.

exceedingly minute amount of the vapour. I estimate that a sensitive mixture of chlorine and hydrogen which contains one molecule of nitrogen chloride to 1,000,000 molecules of chlorine and hydrogen is at least 100 times less sensitive to light than a similar mixture which contains none of the vapour. The inhibitory effect of oxygen discovered by Bunsen and Roscoe is surprisingly large but that of nitrogen chloride is very many times greater. As photochemical changes are so sensitive to the influence of common impurities, it is not surprising that so little progress has been made in the elucidation of the laws which control chemical transformations induced by the agency of light.

It will now be convenient to relinquish for the present the inquiry into the phenomenon of photochemical inhibition, in order that we may pass on to the discussion of a question to which an answer must be found before we can formulate any views concerning the mechanism of the influence of light in promoting certain chemical changes. When white light traverses a mixture of chlorine and hydrogen, we know that some of the rays are extinguished, since the emergent beam is coloured. Is the whole of this abstracted light absorbed by the one coloured constituent, chlorine, without the intervention of the hydrogen; or is a certain proportion of the light extinguished as a result of the chemical change which is proceeding, the amount being proportional to the change? More than one authority has asserted that the latter is the correct view. Bunsen and Roscoe interpret some of their experiments with the aid of the assumption that the absorbed rays can be divided into two distinct parts, those which are absorbed by the constituents of the mixture in virtue of the optical properties of these and those which affect the chemical changes. If this view were correct, a mixture of air and chlorine in equal volumes would absorb less light than a mixture in equivalent proportions of chlorine and hydrogen. Bunsen and Roscoe claim to have shown experimentally that such is the case; but in order to interpret the results of their experiments, they were compelled to assume that a formula which is only strictly applicable to monochromatic light could for all practical purposes be used to interpret the results of experiments performed with composite light. This objection was fully realised by Bunsen and Roscoe at the time. Burgess

and the writer¹ have submitted this important question to re-examination, using an experimental method free from the objection indicated above. Light from Harcourt's standard pentane lamp, after it had traversed a column of a mixture of equal volumes of chlorine and oxygen enclosed at atmospheric pressure in a cylinder with transparent ends, was permitted to fall on the insolation vessel of a Bunsen and Roscoe actinometer containing gas uncontaminated with any destructible inhibitor; the intensity of the light was then determined by measuring the rate of interaction of the chlorine and hydrogen. A mixture of chlorine and hydrogen in equal volumes and at the same pressure was then substituted for the mixture of chlorine and oxygen in the cylinder through which the light passed before falling on the actinometer. The intensity of the light proved to be the same in both cases. A mixture of chlorine with an equal volume of hydrogen is therefore not less transparent than a similar mixture of chlorine and oxygen. The chemical change does not cause light to be absorbed; it is the light absorbed by the chlorine which stimulates the molecules of the two gases to interact. Our conclusion that an absorption of light does not occur as a direct result of a chemical change has been confirmed recently by several workers engaged on investigations relating to other photochemical changes² and is, I believe, now generally accepted as true.

We were now in the possession of two fundamental facts on which to base a working hypothesis. Firstly, the energy which brings about the change is derived solely from the light absorbed by the chlorine in virtue of the selective absorption exercised by the latter; secondly, certain impurities have an enormous effect in retarding the interaction of the chlorine and hydrogen. At the time when these two facts were established, the investigation of R. W. Wood on the resonance spectra of the elements was being carried on and was attracting considerable attention. Wood's work had demonstrated the great complexity of the vibrations set up in the atoms and molecules of the elements by the stimulating effect of light and it was known that these vibrations could be profoundly modified by traces of impurities. Influenced by these

¹ *Journal Chem. Soc.* 1906, 89, 1399.

² Winther, *Zeitsch. wiss. Photochem.* 1908, 8, 242; Weigert, *Zeitsch. Elektrochem.* 1908, 596.

considerations, we put forward the hypothesis¹ that the light which falls on the moist mixture of chlorine and hydrogen is absorbed, in the first instance, by the coloured component (the chlorine) and after it has been absorbed is degraded into heat; during the process of degradation, the energy passes through various forms. Now it is conceivable that the distribution of the various kinds of vibration of which the degrading energy is composed will depend in certain cases largely on the presence in the system of even small quantities of foreign bodies. A difference in the rate of chemical change might clearly be expected as a result of a marked difference in the character of the energy through which the light passes as it is degraded into heat. We shall see below how far this view has been confirmed by subsequent discoveries.

A statement had been made many years before this explanation of the facts was put forward and had remained uncontested, concerning the influence of the proportions of chlorine and hydrogen on the rate of interaction of the gases, which, if true, would have necessitated a profound modification, possibly a complete abandonment, of our hypothesis. It had been announced, both by Draper and by Bunsen and Roscoe, that the most sensitive mixture was one composed of exactly equivalent proportions of the two gases, a slight excess of either having the effect of reducing very appreciably the responsiveness of the mixture to light. Bunsen and Roscoe assert that an excess of three parts of hydrogen in a thousand reduces the rate of interaction from 100 to 37·8 and that one part of chlorine in a hundred reduces it from 100 to 60. This effect required re-investigation, especially as we suspected that there was a source of error in Bunsen and Roscoe's experiment. Our experiments were at first unsuccessful, owing no doubt to the circumstance that the chlorine and hydrogen used to dilute the mixture contained impurities. An appreciable retardation in the rate of formation of hydrogen chloride was brought about by the addition of a small quantity of either constituent to the electrolytic gas but its magnitude was variable. It was only after a method had been devised for the preparation of chlorine and hydrogen quite uncontaminated with destructible inhibitive impurity and containing very little oxygen that the experiments furnished consistent results. These results demonstrated con-

¹ *Journ. Chem. Soc.* 1906, 89, 1433).

clusively that the addition of a small volume either of hydrogen or of chlorine to a mixture of chlorine and hydrogen in equivalent amounts did not appreciably affect the sensitiveness of the mixture.¹ It may here be mentioned that the hydrogen used by Bunsen and Roscoe to dilute the mixture was prepared by the electrolysis of dilute sulphuric acid and probably contained oxygen derived from the electrolyte.

As we have already seen, the power of retarding the photochemical interaction of chlorine and hydrogen had been shown to be a property of two substances, oxygen and nitrogen chloride, the effect of the latter being incomparably greater than that of the former. The question arose, Is the property common in some degree to all substances or is it limited to a special class of gases and vapours related in some unknown way to chlorine? To answer this question, the effect of adding small amounts of a large number of volatile substances to electrolytic gas had to be investigated. Accordingly an apparatus was devised by means of which a measured volume of the gases to be tested could be introduced into the insolation vessel of an actinometer which contained a sensitive mixture of chlorine and hydrogen. A series of experiments disclosed the fact that the inhibitors belong to a special class of substances and that substances outside this class exert an inappreciable influence on the rate of interaction. Moreover, all the substances which were proved to retard the action at all were also shown to be capable of exerting an inhibitive influence of surprising magnitude. The inhibitors discovered were nitric oxide, chlorine peroxide and ozone.² In the case of nitric oxide, it is not certain whether the true inhibitor is nitrosyl chloride or peroxide of nitrogen. On entering the actinometer, the nitric oxide would be converted almost immediately into nitrosyl chloride but this compound would be acted upon, perhaps very rapidly, by the water vapour present and converted into nitrogen peroxide. Nitrosyl chloride undoubtedly retards the interaction of dried chlorine and carbon monoxide; but it is doubtful if it can exist more than a short length of time in the presence of moisture. It is not improbable, therefore, that both nitrosyl chloride and nitrogen peroxide prevent the interaction of chlorine and hydrogen. As might have been

¹ Chapman and MacMahon, *Trans. Chem. Soc.* 1909, 95, 135.

² *Ibid.*, *Trans. Chem. Soc.* 1909, 95, 1717, and 1910, 97, 845.

predicted, the gaseous products of interaction of nitric oxide and chlorine were gradually dissolved by the water in the actinometer, so that the effect of adding the nitric oxide disappeared after several days, even when the insolation vessel was not exposed to light. In this respect, nitric oxide differs in its behaviour from nitrogen chloride, which will remain for months in the presence of moist chlorine and hydrogen and only decomposes at an appreciable rate at a higher temperature or under the influence of light. An idea of the magnitude of the effect of nitric oxide can be gained from the following experiment. A measure of nitric oxide, equal to $\frac{1}{100}$ of the total volume of the mixed gases, was admitted to the insolation vessel: the mixture was exposed to the light of a glow lamp during half an hour, in which period there was no detectable movement of the index. If the original mixture had been exposed to the light during the same length of time, about one-third of the electrolytic gas would have been converted into hydrogen chloride. After the illumination of half an hour, the mixture was allowed to remain in the dark during two and a half hours and then re-exposed to light; during twenty-five minutes there was no interaction. The actinometer was then left during thirteen hours in the dark; on exposure to light there was an instantaneous formation of hydrogen chloride. The movement of the index was at first slow but it gradually increased until the sensitiveness of the mixture was almost as great as that observed before the nitric oxide had been added.

The inhibitory effects of chlorine dioxide and of ozone are comparable with that of the product of interaction of chlorine and nitric oxide. Even in the dark, the ozone completely disappears after a few hours, owing to its extreme instability in the presence of chlorine. The oxygen from the decomposition of the ozone of course reduces the sensitiveness of the mixture of chlorine and hydrogen but is not capable, like ozone, of almost, if not completely, preventing the formation of hydrogen chloride. The destruction of the ozone appears to take place more rapidly in the light than in the dark.

The known inhibitors, therefore, are oxygen, nitrogen chloride, nitrosyl chloride or nitrogen peroxide, chlorine peroxide and ozone. They are all oxidising substances with moderately unstable molecules, the one with the most stable molecule, namely oxygen, being by far the weakest inhibitor. Chemically

inert substances such as carbon dioxide and nitrogen are incapable of reducing the rate of the photochemical process nor has any reducing substance been discovered which possesses inhibitory properties.

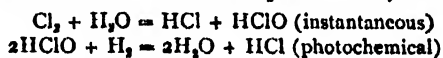
Chlorine monoxide and nitrous oxide, though oxidising agents, exert no influence on the change. That chlorine monoxide should be incapable of modifying the rate of interaction of moist chlorine and hydrogen is not astonishing, as moist chlorine gas almost certainly contains a small proportion of the lower oxide of chlorine: a solution of chlorine in water consists largely of hypochlorous acid and it would be surprising if the vapour of the latter substance were not to some extent dissociated into chlorine monoxide and water vapour.¹

Nitrous oxide, although usually classified as an oxidising agent, since it supports combustion, is probably incapable of parting with its oxygen at the ordinary temperature. There is, in fact, reason to suppose that the molecules of this gas are so stable that it may be regarded as an inert substance except at elevated temperatures.

Oxygen, ozone, nitrogen chloride and nitrosyl chloride also retard the interaction of chlorine and carbon monoxide, the effect of the nitrosyl chloride being in this case permanent, since it is not destroyed by light and no water is present to effect its removal. Luther and Goldberg have shown that oxygen retards the interaction of chlorine and benzene; and in a research which has not yet been published, Mr. R. Atkin has found that some of the other substances which retard the interaction of chlorine and hydrogen act inhibitably towards the union of chlorine and benzene. Each known inhibitor appears to be capable of exerting a retarding influence on all photochemical actions in which chlorine takes part.

We may here draw the attention of the reader to a remark-

¹ Either chlorine monoxide or hypochlorous acid may be an intermediate product in the formation of hydrogen chloride from moist chlorine and hydrogen. The course of the interaction would then be represented by the equations:



The circumstance that an increase in the partial pressure of the hypochlorous acid makes no difference to the rate at which hydrogen chloride is produced cannot at present be regarded as a valid objection to this view, as it is not improbable that the rate of a photochemical change is regulated almost entirely by the rate at which the light is absorbed and degraded.

able parallelism (already indicated by F. H. Gee and the writer) between the phenomenon of photochemical inhibition and that of resonance investigated during recent years by R. W. Wood. Gee and the writer¹ comment on this coincidence in the following terms :

"Concerning the mechanism of the photochemical changes under consideration, our own view is briefly this. Chlorine, when it is absorbing light, preserves, for a time, the transformed light energy in efficient forms which are gradually changed and finally become the ordinary heat energy of the system, the rate of degradation being considerably greater in the presence of certain impurities. This efficient energy confers on the gas the property of reacting with other substances for which it possesses an affinity and therefore the presence of those impurities which hasten the degradation of energy is a circumstance that can only result in a reduction in the rate of a possible photochemical change.

"It might be urged that if efficient energy is accumulated in the chlorine in the manner assumed and that if consequently the light is not instantly degraded to the state in which it exists in the unilluminated system at the same temperature, it ought to be possible to demonstrate the existence of this energy in the illuminated gas by some physical means. The work of R. W. Wood on the resonance spectra of the elements would appear to have a direct bearing on this aspect of the question. Five years ago it was shown by this investigator that iodine—an element allied to chlorine—in the state of vapour emits a green light when the rays from an arc-lamp act on it and that in the presence of small quantities of oxygen the fluorescent light is considerably reduced in intensity. At that time an unsuccessful attempt was made to show that chlorine would fluoresce under similar conditions. Quite recently Wood has returned to the subject² and his latest results are such as to strengthen the conviction that there is a close relationship between the phenomena investigated by him and those observed in the study of photochemistry. He has now shown that, when the pressure is sufficiently low, bromine vapour can be made to fluoresce, a fact which very considerably increases the probability that chlorine, exposed to light rays, will ultimately be shown to be capable of retaining the absorbed energy in an efficient form for a sufficient length of time to give rise to the phenomenon of fluorescence. What is still more significant is the influence of impurities on the fluorescence of iodine vapour. When the vapour is excited by monochromatic light—the green light of

¹ *Trans. Chem. Soc.* 1911, 99, 1727.

² *Phil. Mag.* 1911 [vi], 21, 261, 309 and 314.

mercury—and the fluorescent light is analysed, it has been found to consist of a number of lines, designated by Wood a resonance spectrum. The line spectrum becomes a band spectrum when helium is present in the vapour and at the same time the proportion of light in the red to that in the green is increased. The helium transforms and simultaneously degrades the energy. Wood also finds that after the iodine vapour has been mixed with the electro-negative gases chlorine or oxygen, the degradation is so rapid that the fluorescence can no longer be made manifest. Now all the gases which retard or prevent the interaction of chlorine and hydrogen are likewise electro-negative in character. This close coincidence would be most remarkable if merely fortuitous but if, as we are disposed to think, it arises from a causal connexion between the two classes of phenomena, it could scarcely be disputed that it does afford strong presumptive evidence in favour of the view that photochemical inhibition results from the property possessed by the inhibitor of degrading the energy essential to the progress of the chemical change. The fact (for which this communication contains evidence) that the gases which behave as inhibitors towards the action between chlorine and carbon monoxide are also inhibitors in the case of the interaction of chlorine and hydrogen, lends further support to the same view."

A direct and obvious consequence of the views that we hold on the mode in which light brings about a chemical transformation and on the nature of the influence of certain impurities in modifying the action of the light is that the impurities in question should not diminish the rate of the same chemical change when the action is promoted by merely elevating the temperature and the system is in thermal equilibrium with all the surrounding objects from which it can receive radiant energy. To put this conclusion to the test of experiment, the interaction of chlorine and carbon monoxide was the most suitable and nitrosyl chloride appeared to be the best inhibitor, as it is capable of almost entirely preventing the photochemical action but is not destroyed by light and is stable at the temperature at which the thermal change proceeds with a moderate velocity. An apparatus was constructed in which a mixture of equal volumes of chlorine and carbon monoxide, enclosed in a glass bulb, could be kept at a constant high temperature in an electric furnace and at the same time exposed to light, the velocity of combination being measured in the usual manner by the rate of contraction of the contained gases. With the aid of this apparatus, it was

demonstrated that nitrosyl chloride had no influence on the thermal interaction of chlorine and carbon monoxide but that at high, as well as low, temperatures, it reduced to a negligible value the responsiveness of the mixture to light. The kind of chemical inhibition under discussion is therefore essentially and exclusively a photo-phenomenon. If a substance owed its effectiveness as an inhibitor to its property of combining with an unknown catalyst, instead of to its capacity to modify and degrade the vibrational energy of the system, then we should expect it to retard the thermochemical as well as the photochemical change.

We shall now pass on to the consideration of the bearing on the theory of the relation which has been found to subsist between the partial pressure of the oxygen contained in a mixture of chlorine and hydrogen and the sensitiveness of the mixture to light. It has been shown that if the proportion of oxygen be small, the sensitiveness (*i.e.* the velocity of formation of hydrogen chloride for constant intensity of illumination) is almost inversely proportional to the amount of oxygen present in a given volume.¹ This result is in accordance with the assumptions that the degradation of the vibrational energy which causes the interaction of the chlorine and hydrogen is entirely effected by the oxygen and that the rate of degradation is proportional to the concentration of the oxygen. An interesting and obvious deduction from the experimental result just stated is that if the relation continue to hold for infinitely small concentrations of oxygen, a mixture of chlorine and hydrogen entirely deprived of oxygen would be infinitely sensitive. It has recently been shown that the same relation does not hold between the sensitiveness of a mixture of carbon monoxide and chlorine and the content of oxygen if the value of the concentration of the oxygen be large;² when the partial pressure of the oxygen is relatively great, the doubling of the concentration has very little effect on the sensitiveness of the mixture; thus, in the case of a mixture which contained 25 per cent. of oxygen the sensitiveness was 0.745, whereas in one which contained 50 per cent. of oxygen (the concentration of the chlorine and carbon monoxide being the same) the sensitiveness was 0.733. This result

¹ Chapman, MacMahon, *Trans. Chem. Soc.* 1909, 95, 960.

² Chapman and Gee, *Trans. Chem. Soc.* 1911, 99, 1726.

points to the conclusion that a certain small proportion of the effective vibrational energy is not modified by the oxygen molecules. For lower concentrations of oxygen the relation found to hold for mixtures of chlorine, hydrogen and variable small amounts of oxygen is approximated to. It would appear that, as a first approximation, the sensitiveness of a mixture of chlorine and carbon monoxide (and possibly also of a mixture of chlorine and hydrogen) containing oxygen at different partial pressures is given by the formula $S = A + B/[O]$, in which A and B are constants and S and $[O]$ are the sensitiveness and concentration of oxygen respectively. If $[O]$ be small, S is so large that in comparison A becomes negligible and the relation $S = \frac{B}{O}$ holds within the limits of experimental error;

but if $[O]$ be large, S becomes almost equal to A . Further experiments on the retardation of the photochemical interaction of chlorine and hydrogen by oxygen are now in progress and an attempt is being made to prepare chlorine and hydrogen uncontaminated with oxygen.

It will be seen that some advance has been made in elucidating the nature of the process which takes place when chlorine and hydrogen or carbon monoxide interact under the influence of light; but what is of equal importance is the fact that we are now in possession of sufficient information to enable us to investigate, with reasonable hope of obtaining results in which confidence can be placed, the important question of the effect of the concentration of the interacting substances on the rate of the chemical process.

A number of investigations on the displacement of equilibria by the agency of light have been carried out during recent years. A description of these has been omitted purposely from the present article, our knowledge of the quantitative laws of photochemistry, in the opinion of the writer, being at present too vague and inexact to admit of the results of these researches being discussed profitably.

In conclusion reference may be made to some of the effects of ultraviolet light and the cathode rays. Ultraviolet light is a much more efficient agent in promoting chemical changes than visible light. Under its influence chemical transformations will proceed in colourless gases at an appreciable rate. Light of short wave length owes its high efficiency to two

causes—firstly, to the ease with which it is absorbed by nearly all substances; secondly, to the circumstance that being of higher refrangibility a larger proportion of its energy is available for the performance of work.

In 1894 Ph. Lenard¹ showed that cathode rays which had penetrated an aluminium window in a vacuum tube produced ozone in the air through which they passed. Whether the formation of ozone was due directly to the cathode rays or indirectly to the ultraviolet light produced by the passage of the cathode rays through air is doubtful. Lenard was unable to detect any other chemical changes induced by the action of this form of energy; electrolytic gas did not explode, carbon disulphide did not burn, hydrogen sulphide was unchanged and nitrogen and hydrogen did not interact when subjected to the rays.

Lenard² also investigated somewhat exhaustively the effects of ultraviolet light on gases. He showed, firstly, that under the influence of light gases became conducting; secondly, that condensation nuclei were produced; thirdly, that in the case of oxygen ozone was formed. These effects were brought about in air by light of wave-length 0·00014 to 0·00019 mm., that is, only by the rays of highest refrangibility to which air is comparatively opaque. Hydrogen was more transparent to ultraviolet light than air and accordingly was unaffected by light of greater wave-length than 0·00016 mm. To the most chemically active rays, air at atmospheric pressure was more opaque than rock-salt, fluorspar or quartz. It is important that this relative opacity of air should be borne in mind in the construction of any apparatus to be used in the examination of the chemical effects of rays and that air spaces in the path of the rays should be avoided.

Closely connected with the above-mentioned work of Lenard is an interesting research by E. Warburg,³ in which the discharge of electricity through oxygen from a point was investigated. Under different conditions the amount of ozone produced was from 1,000 to 93 times greater than the amount which would have been found had its production been due entirely to electrolysis. From this fact the necessary con-

¹ *Ann. Physik.* 1894, 51, 225.

² *Ibid.* 1900, 70, 486.

³ *Sitzungsber. K. Akad. Wiss. Berlin*, 1903, 1011.

clusion was drawn that ozone produced in the path of the electric discharge results from the action of ultraviolet light and cathode rays on oxygen, a view which received further support from the circumstance that the amount of ozone formed in a given time was roughly proportional to the intensity of the light.

E. Warburg and E. Regener¹ were the first to demonstrate that ultraviolet light could induce other chemical changes besides the conversion of oxygen into ozone. As a source of ultraviolet light they employed an electric spark between aluminium electrodes. With their apparatus 2.2 per cent. of oxygen at atmospheric pressure could be converted into ozone. They found that ammonia, nitric oxide and nitrous oxide were readily decomposed by the light.

S. Chadwick and J. E. Ramsbottom and the writer have shown that the ultraviolet light emitted by a quartz mercury lamp will bring about the interaction of oxygen and hydrogen or carbon monoxide and effect the decomposition of carbon dioxide into carbon monoxide and oxygen. As might be expected, the presence of moisture has a very marked effect both on the union of carbon monoxide and oxygen and on the decomposition of carbon dioxide. Its accelerative influence on the one change, the combination of the carbon monoxide and oxygen, is so much greater than that on the reverse change, the decomposition of carbon dioxide, that although dry carbon dioxide is decomposed to the extent of 46 per cent. at a low pressure by ultraviolet light, it is scarcely affected by the same agency when it contains moisture. When a carefully desiccated mixture of carbon monoxide and oxygen and a similar mixture in a moist condition are submitted to the action of ultraviolet light of the same intensity, the rate of contraction is the same in both cases; but the contraction in the first case is due mainly to the formation of ozone, whereas in the second it is caused principally by the production of carbon dioxide. A. Holt² has obtained very similar results by decomposing carbon dioxide at a low pressure by the silent discharge; but at a higher pressure the results he obtained were different from those furnished by our experiments with ultraviolet light. He believed that the chemical

¹ *Sitzungsber. K. Akad. Wiss. Berlin*, 1904, 1223.

² *Trans. Chem. Soc.* 1909, 96, 34.

effects of the silent discharge through gases at a low pressure is mainly due to ultraviolet light, whilst at higher pressures other agencies such as the cathodic rays come more prominently into play.

Thorough investigations of the action of ultraviolet light on a mixture of carbon monoxide and steam and on a mixture of hydrogen and oxygen would most probably furnish results of considerable interest, especially as it has been shown recently by W. Wieland¹ that formic acid is produced in appreciable quantity by the interaction of carbon monoxide and steam under certain conditions and F. Fischer and M. Wolf² have found that a very high percentage of hydrogen peroxide may be produced by the action of the silent discharge on a mixture of hydrogen and oxygen.³

DESCRIPTION OF ACTINOMETER

Most of the experiments on the photochemical interaction of chlorine and hydrogen described above can be performed with the aid of the apparatus shown in the accompanying figure. The hydrogen and chlorine are prepared by the electrolysis of concentrated chlorhydric acid contained in the large U-tube on the left of the figure. The electrodes *A* and *C*, of graphite, are fused into hard glass tubes which are ground into the narrow ends *a* and *γ* of the two limbs of the U-tube. The circuit is closed by touching the tops of the two graphite sticks with the copper wires which convey the current. The hydrogen and chlorine generated by the electrolysis of the acid escape through the capillary tubes fused into the necks of the two limbs of the U-tube. By turning the three-way taps *c* and *a* into the right positions either the hydrogen and chlorine can be permitted to escape through the tubes *x* and *y* or conducted through the taps *b* and *d* into the actinometer. The apparatus therefore may be used to furnish a mixture of chlorine and hydrogen in equivalent proportions or to prepare either of the gases separately. The bottom of the U-tube is filled with glass beads to prevent the movement of the saturated solution of chlorine

¹ *Berichte*, 1912, **45**, 681.

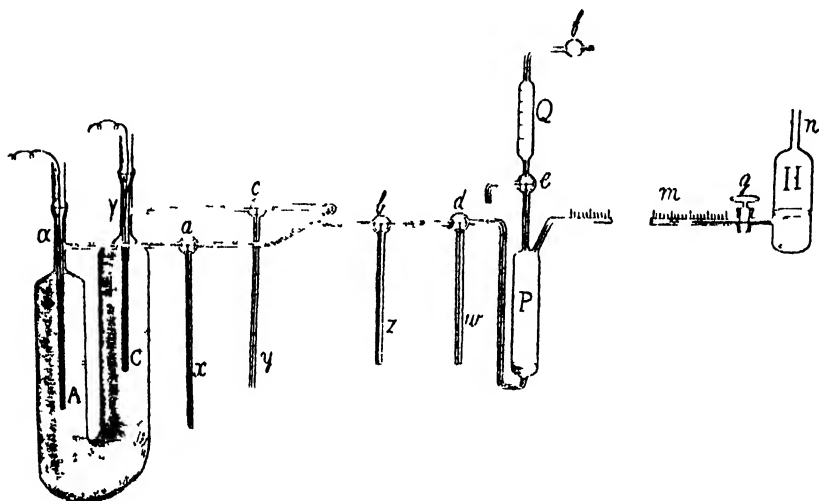
² *Ibid.* 1911, **44**, 2956.

³ Both of these important discoveries are in complete harmony with Armstrong's views on combustion. He has, in fact, predicted that the production of formic acid would be found to be an intermediate stage in the combustion of moist carbon monoxide.

in the anode limb towards the cathode: if this precaution be not taken, the hydrogen evolved at the cathode is contaminated with a large proportion of chlorine.

The gases enter the insolation chamber *P* of the actinometer and after passing through the index tube *m* escape through the water contained in the reservoir *H*. The insolation vessel *P* is immersed in a bath of water provided with a glass window through which it can be illuminated. The water-bath is kept at a constant temperature by means of a delicate thermo-regulator.

When *P* is exposed to light, the hydrogen and chlorine it



contains are converted into hydrogen chloride, which dissolves very rapidly in the water present. The consequent reduction in volume is measured by the movement of the water in the index tube *m* (to which a scale is attached) towards the insolation vessel. In order to keep the pressure in *H* constant, the tube *n* is connected with a large bottle filled with air which is placed in the same water-bath as the insolation vessel.

The graduated tube *Q* is used to add measured quantities of liquid to the insolation vessel *P*. The liquid is drawn through the tube to the left of the three-way tap *e* into *Q*; and *e* is then turned so as to communicate with the insolation vessel only and a measured quantity of liquid is forced by pressure exerted through the tap *f* into the insolation vessel.

If it be desired to investigate the effect of a gas on the rate at which chlorine and hydrogen interact, a given quantity of the gas can be admitted to the insolation vessel by the following procedure: The three-way taps *b* and *d* are turned so that the tube *z* communicates only with the tube *w* and a current of the gas to be used is passed in at *z* and out at *w*; when all the electrolytic gas in the capillary tube between *b* and *d* has been displaced, the three-way taps are turned so that the tubes *z* and *w* are closed and the cell and actinometer are brought into direct communication; the gas contained in the capillary tube between *b* and *d* is then driven into the insolation vessel by means of a current of electrolytic gas.

THE STRUCTURE OF METALS

By CECIL H. DESCH, D.Sc., Ph.D.

THE study of the structure of metals in relation to their physical and mechanical properties is of quite recent origin. Apart from a few isolated observations by Hooke and Réaumur, the first to use the microscope in investigating metals was H. C. Sorby, the brilliant Sheffield amateur who was a pioneer in so many departments of research. The method of preparing and examining metallic specimens devised by Sorby in 1864 is in all essential respects the same as that in general use at the present time, notwithstanding the many important improvements of detail introduced by later workers. His unwearied patience and skill in applying the microscope to the study of iron and steel were attended with remarkable results; nevertheless, his work passed almost without notice and nearly twenty years elapsed before any general attention was given to the subject. Since that time, the advance of microscopical metallography has been rapid and continuous, in regard both to the number of workers and to the methods of investigation and interpretation.

The subject of metallography is not confined to the study of metals and alloys by means of the microscope but includes investigation by thermal, electrical, mechanical and other methods into which it is not proposed to enter now.¹ Reference must also be made to text-books for details of the technique of preparing and examining sections, merely noticing that, owing to their opacity, metals have always to be examined by reflected light. The object of the present article is to describe some of the more important conclusions already established concerning the internal structure of the principal metals and alloys of technical importance and the connexion between structure and practical utility. Appreciation of the value of the

¹ See, for example, W. Guertler, *Metallographie*, 2 vols., now in course of publication (Berlin: Gebr. Borntraeger), or C. H. Desch, *Metallography* (London: Longmans, Green & Co., 1910).

microscopical method is no longer confined to investigators in the fields of inorganic and physical chemistry but is becoming general among manufacturers and users of metals, examination by means of the microscope now forming an essential part of the routine in a large and increasing number of metallurgical and engineering works.

The structure of solid metals is, in the main, crystalline. Of cast and slowly cooled or annealed metals this is probably strictly true, whilst rolled, drawn or otherwise cold-worked metals are built up of material which is only in part crystalline and in part glassy or amorphous. This difference of structure gives rise to important differences of properties between the two materials and it will be convenient to consider metals and alloys in the thoroughly crystalline state before passing on to the modifications brought about by mechanical work.

Technically, metals (using the term in its widest sense, to include alloys) may be divided into two classes, from the point of view of structure, namely, those which are homogeneous throughout and those which are composed of two or more distinct crystalline constituents. The first class includes the pure metallic elements and also a much more numerous group of alloys, whilst the second class includes all other alloys. The manner in which crystallisation is effected in all members of the first class is essentially the same and a description of the process may serve as an introduction to the general problem.

The passage of a metal from the liquid to the solid state, like that of any other crystalline substance, does not take place simultaneously throughout the mass but begins at certain nuclei, the number and distribution of these depending on the nature of the substance and on the conditions of cooling. The number of nuclei is greater when the liquid is cooled considerably below its freezing-point before solidification begins than when undercooling is reduced to a minimum. It has been suggested that the number is also dependent on the degree of heating to which the liquid has been previously subjected but there does not seem to be experimental justification for such a view, which is one also that it is difficult to accept on theoretical grounds. The chief determining factor is certainly the degree of undercooling.

The nuclei having once appeared, the further deposition of solid matter takes place around them as centres; not, however,

equally in all directions, even when the temperature is maintained as uniform as possible. The growth of a crystal in a mass of fused metal does not, as a rule, resemble the growth of a crystal of chrome alum in an undisturbed solution. Instead of a perfect or almost perfect octahedron being formed by the gradual addition of layer to layer, so that the shape is preserved as the crystal increases in size, the accretion of solid metal takes place principally along certain axes, a skeleton or crystallite being formed. This behaviour is characteristic of metals.

In the case of certain salts, according to O. Lehmann,¹ the first visible mass surrounding the nucleus may be an octahedron; during the subsequent growth the added matter is not deposited uniformly over the surfaces of the octahedron but becomes attached chiefly at the solid angles, so that the particle becomes star-shaped. Further growth at the now sharpened angles accentuates the difference from an octahedron, the form of which soon disappears, its place being taken by needle-like prolongations of the axes. The effect has been satisfactorily explained by Lehmann, in the cases examined by him, as being due to the rate of growth exceeding that at which the supersaturation or undercooling in the immediate neighbourhood can be equalised by diffusion or convection; it is not clear that the same explanation will serve for slowly crystallising molten metals. Whatever the cause may be, the skeletal mode of growth is more frequent in metals than the normal mode of accretion by successive layers.

Before the prolongations of the axes attain to any great length, secondary axes make their appearance, in the form of transverse growths parallel with the other axes of the original crystalline particle; these are followed in turn by tertiary axes and others of a higher order. The skeleton therefore becomes more complex and more closely packed, approaching more and more nearly to a compact mass. Given a sufficient supply of liquid metal, the process of "filling up" continues until the numerous axial growths are in perfect contact and the mode of formation of the crystallite has ceased to be apparent. If, however, the supply of liquid be restricted or if the closing up of the outer parts of the crystallite be complete before the inner part is solid, cavities may be left which afford

¹ *Molekularphysik*, i. 326 (Leipzig, 1884).

an indication by their form and distribution of the original axial arrangement.

The growth in length of the axes and consequently the growth in volume of the crystallite is not limited by the development of external crystal faces but simply by the interference of neighbouring crystallites. The mass of solid metal is ultimately composed of polyhedral "grains"; each of these represents the growth about a single primary nucleus, whilst the degree of uniformity of their dimensions is an indication of the regularity of distribution of the nuclei. The grains are the units of crystalline structure in a homogeneous metal or alloy. Their boundaries appear as polygons in a plane section through the solid metal.

If we consider the common case of molten metal cooling in an ingot mould, it is evident that the temperature of the mass will fall most rapidly at the outer surfaces. The first nuclei therefore make their appearance in contact with the walls of the mould before the layers at a greater depth are sufficiently undercooled to allow solid matter to separate. In consequence of this distribution of the nuclei, the first crystallites grow inwards from the surface. If the conditions of cooling are uniform, these crystallites are approximately equally spaced and tend to grow as parallel, elongated, more or less prismatic masses recalling to a botanist the form of the "palisade parenchyma" of a leaf. In a small ingot or in one which has cooled with extreme slowness, these parallel crystallites may extend so far inwards as to meet in the middle, whilst in larger ingots or under more usual conditions of cooling they merely form an outer layer, the interior being made up of smaller crystallites without parallel orientation.

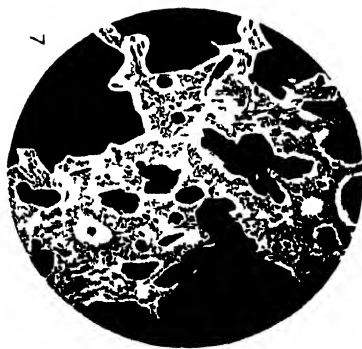
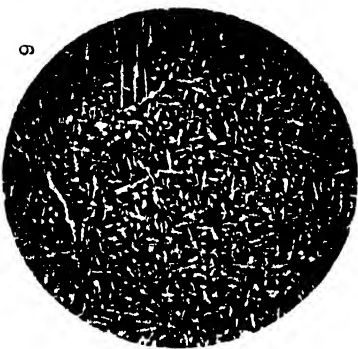
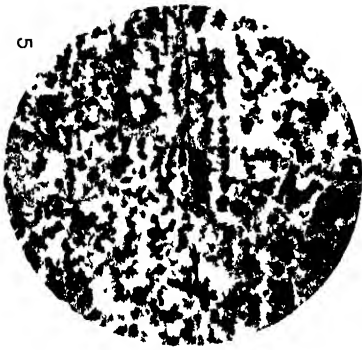
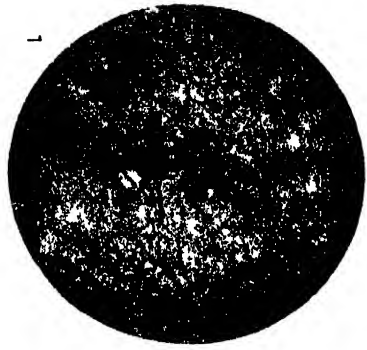
The typical structure of an ingot of pure metal as seen in a transverse section is, then, a number of irregular polygons, of which the outermost are parallel to one another and perpendicular to the faces of the ingot, whilst those in the interior are of approximately equal size and are not developed in any chief direction. Naturally, as there is no chemical difference between any one part of the section and any other, the structure is not seen in a section which has merely been cut and polished but in order to reveal it etching with a corrosive agent is necessary. Thus, for example, a surface of copper may be etched with nitric acid. The copper is attacked and its surface is roughened.

Under a high magnification the roughening is seen to be due to the formation of very numerous "etch-figures" or hollows of geometrical outline; the form of these serves to give information as to the crystalline system to which the metal belongs. Within any one grain, the arrangement of the etch-figures is strictly parallel but the orientation varies from grain to grain, the result being that when light falls on the etched surface it is reflected at different angles by different grains, so that one may appear light and another dark in the field of the microscope. The boundaries of the grains thus become visible as boundaries of light and shade. Another circumstance contributes to render the structure visible. Etching takes place more rapidly at the boundaries than elsewhere, so that after a short time the grains are separated by grooves which become broader and deeper on longer etching. The cause of this phenomenon is not quite clear. Traces of impurity would tend to accumulate at the bounding surfaces of the polyhedral grains and would be removed by etching; but the effect is produced in the most carefully purified metals. It is most probable that the acid acts with different degrees of rapidity along different planes in the crystal—the fact that etch-figures are formed, indeed, points to such a conclusion—and the junction between two grains of different orientation may thus give rise to a difference of electrolytic potential which is small but sufficient to produce an increased action at the boundary. The photograph of iron containing only very small quantities of impurities ("American ingot iron," really a mild steel almost free from carbon) shown in fig. 1 is a typical example of the structure obtained on casting a homogeneous metal. The etching has been so light that the surfaces of the crystal grains have hardly been roughened and the structure has only been rendered visible on account of the etching at the junctions of the grains producing a fine groove which is visible as a dark boundary line.

If, instead of a single metal, the mass under examination be an alloy, cases may occur in which the structure observed in a slowly cooled ingot does not differ from that just described. Yellow brass, containing 70 per cent. of copper and 30 per cent. of zinc, is an example of such an alloy. The brass contains only a single micrographic constituent, as the copper is capable of retaining the whole of the zinc in a state of uniform admixture. Apparent homogeneity in each crystal grain is reached,

however, only slowly; in specimens which have been cooled comparatively rapidly from the molten state, as under ordinary casting conditions, a distinct structure is visible under the microscope. A section of an ingot of brass of this composition is shown in fig. 2. The irregularly polygonal boundaries of the crystal grains are seen as before but the area within each grain, instead of being entirely uniform, as in the ingot iron, is marked with "dendritic" patterns which are evidently of the nature of the crystal skeletons described previously. They are visible in the brass, although invisible in the iron, because the alloy freezes in a manner which is somewhat different from the freezing of a pure metal. The first particles of solid which crystallise from the molten alloy are relatively richer in copper than the liquid and the subsequent accretions to the original nuclei contain a diminishing proportion of copper. There is thus a distinct difference of composition between the material of which the primary and secondary axes are composed and that with which the gaps between the axes are filled up. If an etching-agent be used which attacks the portions richest in zinc most readily, the parts of each crystal grain which are in contact with the boundary are most etched and appear dark, whilst the central axes appear as light "cores." This cored structure is characteristic of cast homogeneous materials, including brass, gun-metal and many of the special engineering alloys. Theory teaches us that equilibrium is only reached when the composition of the mass is rendered uniform throughout by diffusion of one of the constituents from places of high to those of lower concentration. This diffusion, however, has to take place in a solid the internal viscosity of which is very great and the equalisation of composition is therefore a slow process. Annealing the alloy at a sufficiently high temperature greatly facilitates diffusion and a specimen of the same brass after thorough annealing exhibits a perfectly homogeneous structure in which no cores are to be seen. Fig. 3 represents the same specimen as fig. 2 after heating to redness during several hours. The light and dark areas are of the same composition and differ only in orientation.

Mechanical work produces a great distortion of the crystal grains in metals and alloys of the above class and the outlines of the broken and distorted grains may be barely distinguishable in a thoroughly worked metal. Annealing brings about a



1, In₂O₃ Ir 2 70.50 Br 3 70.50 Pass 4 German Silver roll 5 70.50 metal 6 70.50 7 70.50 8 70.50

recrystallisation of the deformed material and a return, in great measure, to the original structure of the casting. The formation of new crystals in the worked material, like the original process of solidification, sets in from distinct centres or nuclei and spreads outwards until the crystalline growths from neighbouring centres meet and interfere, giving rise to crystal grains as in the original process of solidification but the complex interlocking of boundaries, which is so conspicuous a feature of many cast metals, is less usual after annealing and an approach to simple rectilinear polygonal forms is noticeable in most worked and annealed metals, especially when they have been subjected to a high temperature. If the metal be worked mechanically before annealing, the crystals that are produced are not simple but frequently twinned, the repeated twinning being similar in effect to that observed in feldspars in rock sections. Fig. 4 represents a rolled and annealed specimen of German silver, a homogeneous mixture of copper, nickel and zinc; both the rectilinear boundaries of the crystals and the repeated twinning planes are apparent.

Another class of alloys, although crystallising from the molten state in the form of a homogeneous solid, as in the metals just described, undergoes such further changes in the solid state that an entirely new structure is produced. To this class belong most of the varieties of steel. All steels solidify in the first instance in the form of crystal grains of uniform composition, if certain minor impurities be, for the moment, neglected. It is, however, rare that such a structure persists during the process of cooling down to the ordinary temperature. Manganese steel, containing 13 per cent. of manganese and 1 per cent. of carbon, which finds such important applications, on account of its resistance to abrasion, in crushing-machinery, tramway crossings, etc., is an example of a steel which retains its polygonal structure permanently; but this is quite an exceptional case. Ordinary carbon steels, from the softest structural material to the hardest varieties of tool steel, have undergone transformation to a greater or less extent, so that the original polygonal grains have been more or less resolved into a complex structure the principal constituents of which are *ferrite* (iron alone or uniformly associated with small quantities of silicon, manganese, phosphorus and other elements but not carbon) and iron carbide or *cementite*, Fe_3C .

In a typical mild steel the mass of the metal is composed of grains of ferrite between which lie patches of a material which appears homogeneous under a low magnification but is really an intimate mixture of ferrite and cementite. Fig. 5 represents a section of a mild steel plate cut in the direction of rolling and etched to show the structure. The arrangement of the grains of ferrite is seen to follow the direction of rolling, whilst the intervening patches of conglomerate are not sufficiently magnified to reveal their internal structure. The manner in which the pearlite and cementite are intermixed in this conglomerate varies with the heat-treatment to which the steel is subjected. In steels quenched from a high temperature, the carbide is in a state of ultramicroscopic subdivision termed "emulsified carbide" by Arnold. It then becomes black on etching and is commonly called *troostite*. If the cooling be less rapid, the carbide becomes coarser and a granular conglomerate, termed *sorbite*, is obtained. This condition is favourable to toughness and is preferred in steel rails. Thoroughly annealed steels contain the iron and carbide in a very finely laminated form, like the surface of some diatoms or of mother-of-pearl and hence termed *pearlite*. This, although generally regarded as the typical condition of the conglomerate, is not physically stable and if the annealing process be prolonged, the laminæ break up, the cementite becomes gathered into relatively coarse granules, segregation continuing until the original finely divided mixture has disappeared entirely and the steel no longer contains any constituent but ferrite and isolated masses of cementite. As each of these structures corresponds with a distinct set of physical and mechanical properties, the importance of the microscopical examination of steel used as a structural material is obvious.

A further example of the breaking-up of a homogeneous solid during cooling may be taken from the alloys of copper with zinc containing about 40 per cent. of the latter metal, to which Muntz-metal and manganese bronze¹ belong. Like the

¹ The necessity of a more systematic nomenclature of alloys is clearly seen in this instance. Bronze is historically and in general usage an alloy of copper and tin. Manganese bronze, however, is an alloy of copper and zinc to which a minute quantity of manganese has been added to remove oxygen. Manganese may be absent from the finished metal. Such absurdities are frequent in the current technical nomenclature of alloys.

lower brasses, these alloys solidify in the first place in the form of a homogeneous mass of crystals but as the temperature falls changes take place in the solid, much as in the case of an ordinary solution, new materials separating out. In this instance the separated material is itself a homogeneous solid containing relatively more copper than the original crystals. By a convention which has been generally adopted in the case of this and similar alloys, the new crystals are designated the α -constituent, the prefix β being assigned to the material of the original crystals and to that part of the "mother crystals" which remains after complete separation of the excess. Alloys of this class have therefore a duplex structure, the α -crystals being outlined on a (fig. 6) background of β . As the proportion of zinc in the alloys is increased, so the proportion of α -crystals diminishes, until alloys containing nearly 50 per cent. of zinc consist of homogeneous crystals of the β -constituent, which may be distinguished from the α -crystals of which 70:30 brass is composed by its different behaviour towards etching-agents and by the absence of the cores which are so characteristic of brass in the cast condition. A further increase of the proportion of zinc beyond 50 per cent. brings about the appearance of small bluish-white crystals of the γ -constituent, which composes the whole alloy when 61 per cent. of zinc is present. This substance is undoubtedly a definite compound, Cu_2Zn_3 . It is exceedingly brittle and its presence, even in small quantities, is fatal to the good mechanical properties of the alloys. The proportion of zinc which may be alloyed usefully with copper is therefore limited.

Each of these constituents has its special characteristics. The α -crystals are remarkably tough and may be subjected to very severe mechanical deformation without cracking. This property reaches its maximum in the 70:30 alloy, which is frequently known as "cartridge brass" from its use in the manufacture of cartridge-cases in which process it is very severely deformed by forcing through dies. The β -crystals are less tough and ductile but have a higher tensile strength; they are malleable at a high temperature, a property which is not inherent in the alloys richer in copper. The presence of a small quantity of the β -form is essential if the alloy is to be rolled while hot.

Both the α - and the γ -crystals are reincorporated to a very

considerable extent by the β -constituent when the temperature is raised. By heating and rapidly quenching, therefore, most of the alloys of this series having a duplex structure may be rendered homogeneous. Such treatment increases the tensile strength of the alloys in question at the sacrifice of much of their ductility. The quenched alloys are in a more or less unstable condition and the duplex structure is restored by annealing at a moderate temperature.

Aluminium forms alloys with copper which, in some cases, resemble in a very striking manner those containing zinc but a smaller quantity of aluminium is required to produce the effect. Thus the proportions of the α - and β -constituents in an alloy of 60 per cent. Cu and 40 per cent. Zn are almost the same as in an alloy of 90 per cent. of copper and 10 per cent. of aluminium. The latter is an alloy of very high technical value and is well known under the name of aluminium bronze. As in the case of the zinc alloys the γ -constituent, which appears when the aluminium exceeds 12 per cent., is brittle and its presence is fatal to the mechanical properties of the alloy.

The true bronzes are alloys of copper with tin to which smaller quantities of other elements are very frequently added. The α -constituent richest in copper resembles in all essential properties the corresponding alloys with zinc and with aluminium. Most technical tin bronzes, however, contain a small proportion of the β -constituent at high temperatures; as the temperature falls the β -crystals become unstable and are resolved into a characteristic complex of finely divided α and a hard, brilliantly white substance, the δ -constituent. Small areas of this complex occur in many bronzes and form a large part of the hard bronzes used for bearings. One of these areas is shown in fig. 7.

The resolution of the β -constituent of tin bronzes into a complex, which takes place on cooling below 500° and proceeds rapidly to completion, has a remarkable parallel in the alloys of copper and zinc, it having been shown quite recently¹ that the β -constituent in this case also is unstable when cooled below 470° , being resolved into a complex of α and γ . The main difference lies in the velocity of transformation and of recrystallisation. Even when the development of heat during cooling has indicated that resolution into two constituents has taken place, the products remain for some time in a state of such

¹ H. C. H. Carpenter, *J. Inst. Metals*, 1912, 7, 70.

extremely fine division as to be at the limits of microscopical vision and prolonged annealing is necessary in order that segregation may proceed far enough to give rise to a visibly duplex structure. This interesting discovery has thrown much light on the changes of properties undergone by these alloys during heat-treatment and serves further to call attention to the fact that the simplicity of constitution of some of our best known alloys is only apparent and that subjection to long annealing processes at a comparatively low temperature may produce very far-reaching modifications of structure. In view of the extensive use of alloys for engineering purposes in positions in which they are exposed to the prolonged influence of temperatures above that of the atmosphere, the technical importance of this and similar observations is obvious.

A large proportion of the alloys in general use thus fall into one of two classes from the point of view of crystalline structure. The first class comprises alloys in which crystals of a single type compose the whole of the alloy, which has thus, at least in the annealed condition, the structure of a pure metal. This class includes the true brasses, the alloys of copper with small quantities of nickel, arsenic, manganese, iron and other metals, used whenever toughness and resistance to high temperatures are required, as in the fire-boxes of locomotives, the lower tin bronzes, etc., Monel-metal (an alloy of copper and nickel, with the latter in excess), German silver, manganese steel, nickel steel and many other alloys, including the standard gold and silver used for coinage. The second class, in which two types of crystalline material are necessarily present as structural constituents, includes Muntz-metal and manganese bronze, the principal aluminium bronzes, naval brass and other similar alloys. In most gun-metals and in bearing-bronzes, the one material during cooling undergoes resolution into other constituents and is therefore present as a complex. This is also the case with carbon steels.

The class of alloys so frequently encountered in laboratory investigations, in which the primary crystals are surrounded by an eutectic alloy,¹ is relatively of much less importance in technical

¹ An eutectic alloy is an intimate mixture of two or more kinds of crystal characterised by the fact that its melting point is lower than that of alloys containing more of either the one or the other constituent and that it solidifies at a definite temperature.

practice. The most familiar technical examples occur amongst the "white metals" used for the lining of bearings. The essential qualities of such an alloy are sufficient hardness to resist the rubbing action of the shaft and sufficient plasticity to enable the lining to become adapted to the rubbing surface and thus to correct any slight error of alignment or want of accuracy in the shaping of the bearing originally present. These two requirements are best met by an alloy in which primary crystals of some hard material are embedded in a comparatively soft and plastic ground-mass. The hard crystals are generally either of antimony or of a compound of tin and antimony, SnSb , which forms very well-defined crystals of apparently cubical shape. The plastic mass is an eutectic, generally, although not always, containing lead as one of its components. Bearing-metals usually contain more than two metals and a hard and brittle compound of copper and tin is frequently present in small quantities.

A different plan is adopted in the manufacture of "plastic bronzes," which also find considerable application as bearing-metals. In these alloys copper hardened by the addition of either nickel or sulphur or of both forms a sponge the interstices of which are filled with lead. Some tin is added to produce partial miscibility in the liquid state but even with this addition the alloy needs to be cast under specified conditions to avoid separation into two layers. Crystalline outlines are entirely absent from the micro-sections and the structure is merely that of a meshwork of the harder metal holding globules of the soft lead alloy. Such emulsion-like solids are quite unmistakable when seen under the microscope.

Non-metallic elements only enter into consideration as essential structural constituents in a few cases. The most familiar of these is graphite in grey cast-iron or pig-iron. The graphite is seen in the form of thin plates, usually curved and appearing as lines where cut by the plane of the section. The size of the plates and their distribution through the iron give much information as to the mechanical properties that may be expected from the material. A very finely divided variety of graphite is met with in malleable castings as a product of decomposition of the carbide. It is often regarded as amorphous carbon but has been shown to be chemically identical with graphite. Phosphorus is not visible in steel or in ordinary

phosphor-bronze, the minute quantity which is actually present being completely masked but ordinary grey cast-iron contains an appreciable quantity of phosphorus in the form of iron phosphide, Fe_3P , which is distinctly visible as a brilliantly white constituent disposed in characteristic reticulated patterns which represent the eutectic alloy that is the last portion of the cast-iron to solidify on cooling from the molten state. Other non-metallic elements occur principally as impurities and are therefore considered below.

The types briefly enumerated above comprise nearly all the principal metals and alloys encountered in engineering practice, with the exception of white pig-iron which has an eutectic structure peculiar to itself—and of hardened steels—the complications of which are too intricate for discussion within the limits of a short article. The variety in this instance is due to the fact that hardened steels are not in a condition of chemical and physical equilibrium and that many stages may be recognised in the return to the stable condition. It is possible by examining a polished and etched surface of such a steel to form an accurate judgment of the heat-treatment to which the specimen has been subjected. The newer “high-speed” tool steels, containing chromium and tungsten or molybdenum as essential constituents, have structures differing considerably from those of carbon steels and present difficulties of interpretation that have not yet been overcome.

A metal or alloy which has been subjected to heat treatment bears in its internal structure a record of its immediate history and the interpretation of the record is one of the most important applications of metallography to technical practice. As an example, the influence of annealing on the microscopic structure of mild steel may be considered. The temperature at which annealing has taken place may be inferred, other things being equal, from the average size of the crystal grains. It has been found¹ that the rate of growth of the ferrite grains is a maximum at slightly above 700° , growth being less rapid either above or below that temperature. Prolonged annealing at 700° produces an extremely coarse grain. When the proportion of carbon is higher, as in the rail steel, containing 0.40 per cent. of carbon, the ferrite forms “cells,” filled with sorbite or pearlite.

¹ J. E. Stead, *J. Iron and Steel Inst.* 1898, i. 145; A. Joisten, *Metallurgie* 1910, 7, 456.

The size of these cells is a measure of the heat-treatment which the steel has undergone. This is explained by the behaviour of such steel when heated above the recalescence point of 690° . At a high temperature, the iron-carbide complex (sorbite or pearlite) acts as a solvent for the ferrite of which the cell-walls are composed; the crystal grains thus produced grow, like the grains of pure iron, during the annealing process. When the steel is again cooled, the excess of ferrite is no longer held in a homogeneous condition and becomes visible in the first instance at the boundaries of the grains. The size of the cells is an indication of the size of the crystal grains present at a high temperature and is therefore either a measure of the temperature at which the steel has been annealed or, if that be known, of the time during which the metal has been exposed to that temperature. Further, the thickness of the cell-walls is an indication of the rate of cooling, as the first deposition of ferrite takes place at the boundaries of the original grains and any ferrite subsequently deposited must appear in scattered granules within the cell if cooling be rapid but become attached to the cell-wall as an internal thickening if sufficient time be given to allow of free diffusion through the solid mass; a thin cell-wall is therefore evidence of rapid cooling.¹ If the composition of the steel and especially its carbon-content be known, an inspection of the micro-sections gives a complete knowledge of the heat-treatment to which the steel has been subjected, knowledge which is of the utmost value when rails are concerned, the relationship between heat-treatment and the physical and mechanical properties on which the life of the rail depends being now well known.

The deposition of any substance present in excess during the cooling of a homogeneous solid along the boundaries of the crystal grains is not peculiar to steel. It is also observed in alloys of the Muntz-metal class. An alloy of this kind, heated to such a temperature as to be wholly or almost wholly converted into the β -constituent, has crystal grains of a size which depends both on the time and temperature of annealing. During cooling the α -constituent crystallises at the boundaries of the grains and the extent to which thickening of the cell-walls takes place by diffusion depends on the rate of cooling.

The last point to be considered in the present article is the

¹ See H. M. Howe, *Internat. Zeitsch. Metallographie*, 1912, 2, 13.

influence of impurities on the structure. The most easily recognised impurities are those which are not to be regarded as true constituents of the alloys but rather as foreign matter mechanically entangled. Dross in badly made brass is of this character and some other metallic oxides often occur as mechanical impurities. Thus molten aluminium becomes covered with a peculiarly tough and resistant film or pellicle of alumina which is not readily eliminated in the preparation of aluminium alloys by fusion. Remelting is frequently necessary to remove these films. Crystalline stannic oxide remains obstinately entangled in molten tin bronze which has not been sufficiently protected against oxidation and naturally is a cause of brittleness. A slightly different position is occupied by the slag and sulphides found in iron and steel, the impurities in this case being liquid instead of solid at the moment of entanglement in the molten metal. Masses of silicate slag, drawn out into fibres in the direction of rolling, are characteristic of wrought-iron bars, whilst oval globules of grey manganese sulphide are found in mild steel, as in the middle of the field in fig. 5. In the absence of manganese, however, the sulphur in steel is present as ferrous sulphide, which has much less tendency to agglomerate into such oval masses and is commonly met with in the far more dangerous form of thin films separating neighbouring crystal grains. Steel containing ferrous sulphide is invariably red-short so that microscopic cracks are developed in it during rolling.

Passing now to those impurities which are truly alloyed with the metals under examination it is evident that elements which become associated homogeneously with one or the other of the primary constituents cannot be immediately detected by the microscopical method, although occasionally their presence may bring about some perceptible change in the character of the crystals. For example, manganese is miscible with iron and manganese carbide with iron carbide, so that the structure of a mild steel is unchanged by the introduction of manganese. On the other hand, when the manganese is very much increased in quantity, as in certain rich varieties of pig-iron, the increased coarseness of the carbide crystals due to its presence gives a characteristic aspect, both to the etched sections and to the fractured surface, although no new structural constituent has made its appearance.

If an impurity be present as a distinct constituent, its detection by means of the microscope is not difficult. The case of copper may be taken as an example. Highly purified copper, such as is used for electrical purposes, exhibits the typical structure of a pure metal. If, as is usually the case, it has been rolled and subsequently annealed, the crystals are polygonal with almost straight boundaries and show repeated twinning. There is perfect contact between neighbouring crystals. A small quantity of iron, nickel or arsenic does not alter this structure appreciably but a very different effect is produced by sulphur or oxygen. The sulphide or oxide is visible in a polished section in the form of minute globules, which have a characteristic blue colour by reflected light and are therefore readily seen against the red background without the application of any etching-agent. The examination is most easily performed in the case of the cast metal. The fusible eutectic, which is the last portion of the metal to solidify, is then a mixture of copper with either cuprous oxide (Cu_2O) or cuprous sulphide (Cu_2S) and occupies spaces between the crystals. The eutectic, when present in any considerable quantity, takes the form of globules or elongated rods of the oxide or sulphide, the intervals between these being filled with copper. In the micro-section, therefore, a dotted pattern is seen between the crystals. As the proportion of impurity becomes less, the eutectic occupies a smaller area and is at last only recognisable as a narrow, discontinuous layer of globules at the boundaries.

It sometimes happens that the eutectic alloy of a series contains so little of the less fusible metal as to be practically indistinguishable from the second metal. This is the case, for example, in alloys of copper and bismuth. The eutectic point lies so near to the bismuth end of the series that no structure whatever can be detected in the most fusible portion of the alloy, which has the properties of bismuth almost entirely free from copper. Hence, an examination of copper contaminated with bismuth but free from oxygen reveals crystals of copper, usually much reduced in size, separated by a thin film of bismuth, as in fig. 8. It is evident that the presence of such a highly brittle impurity, forming almost continuous layers between the crystals of the copper, must be a source of great mechanical weakness; in point of fact, the specimen represented

cracked at the edges when an attempt was made to hammer it out into a disc long before a specimen of pure copper would have shown signs of failure. The effect of impurities on the mechanical properties of copper is profoundly modified by the simultaneous presence of oxygen, a fact well known to metallurgists.

The detection of impurities is thus a very important part of the work of the metallographist and the chemical and microscopical methods supplement one another in a most valuable way in indicating the properties that may be expected from a given metal or alloy. It must not be forgotten, however, that microscopical examination also gives information which it is not in the power of any chemical analysis to yield—namely, in respect to the heat-treatment that a metal has undergone, on which its physical and mechanical properties so largely depend. Widely different results may be obtained from two specimens of identical chemical composition but the microscopical method seldom fails to throw some light on the difference. Naturally, the relation between structure and properties has not been by any means equally determined in the case of all alloys and there are still many obscure and uncertain points in the method. But both the technical details of manipulation and the establishment of definite relations are advancing rapidly and the microscope is becoming more and more indispensable in all departments of metallurgy. Familiarity with the method is necessary in order to utilise its indications and it is only possible in a short notice to touch upon a few prominent points. The highly important subject of the effect on metals of mechanical deformation is reserved for a second article.

THEORIES AND PROBLEMS OF CANCER

PART II

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IN order that the nature of the investigations dealt with in these articles may be clear to the general reader, it is necessary to say something about the character and varieties of malignant growths. As was pointed out in the previous article, the cells produced by the division of the ovum and subsequent generations of cells become arranged into two layers known as epiblast and hypoblast; groups of cells produced afterwards, situated between these two layers, are known as the mesoblastic layer. Different kinds of tissue are produced from these three layers of cells. The skin is formed from epiblastic cells, the lining of the alimentary canal from hypoblastic, the muscles and bones from mesoblastic cells. Malignant growths may occur in tissues composed of any of these three classes of cells; they are divided, however, into two great groups, *carcinomata*, which arise in epiblastic or hypoblastic cells and *sarcomata*, which arise in mesoblastic cells. Carcinoma includes epithelioma, which is probably what was originally known as cancer. It is practically question that all carcinomata are of the same nature. Carcinoma is essentially a disease of middle and old age; sarcoma occurs chiefly in young individuals and children may be born with it in an advanced stage. Authorities who have studied the matter and are competent to judge are now agreed that the phenomena involved in both carcinomata and sarcomata are essentially similar in character and that like problems have to be faced in either case. It seems probable that the real difference is that one class of tissue is more subject to certain changes at one period of life, the other class at another period.

Abnormal growths of tissue—collections of cells—may be roughly divided into two classes, benign and malignant. In text books it is stated that one of the essential differences between

these two classes is that malignant tumours tend to recur after removal by operation, whereas benign tumours do not. This statement is very misleading. Malignant tumours usually have no well-defined margin and the cells composing them tend to escape along various channels to surrounding or even distant parts of the body; therefore, at no stage can the surgeon be certain that he has removed the whole of the cells which form part of the malignant growth: the so-called recurrence is really a multiplication of cells that have been left behind.

Another feature of malignant growths is the formation of secondary tumours—metastases—in some other part of the body, brought about by cells of the primary growth having travelled and multiplied in a new position. The cells of these secondary growths partake, in a marked degree, of the characters of the cells of the primary growth.

There can be but little doubt that there is sometimes an insensible transition from benign to malignant tumours and that it is impossible to say, at what particular time, in any given case, a change from one to the other took place.

Malignant growths produce no primary symptoms in the persons in whom they occur. All the symptoms and all the damage produced by them are of a secondary nature, due to pressure or some other mechanical action upon surrounding parts of the body.

Having cleared up these points, I will proceed to deal with the possibility of a specific parasite being the cause of cancer and with the present condition of cancer research.

THE PARASITIC THEORY

The discovery that so many diseases are due to micro-organisms entering the body and multiplying there very naturally led to a supposition that cancer was due to a similar cause. The parasitic theory was most popular in the early nineties but since then its adherents have diminished in numbers with ever-increasing rapidity. It may be said at once that very many "discoverers" of the cancer parasite have not had the necessary knowledge and skill to conduct the investigations they have entered upon and that a consideration of their published work is neither profitable nor interesting. On the other hand, men of acknowledged competence have strongly advocated the parasitic theory, though, as James Ewing says: "The

whole basis, objective and theoretical, of the cancer parasite has been traversed again and again with the uniform conclusion by those who have finished the journey that the cancer parasite is the cancer cell."¹ One of the only consistent and highly competent exceptions, as far as I know, is Borrel²; since he admits that the fact that cancer can be taken from one individual and grafted upon another proves nothing in favour of the parasitic theory; it is difficult to see, however, why he still adheres to the idea of a parasite.

The motley throng which has in the past claimed the discovery of the parasite of cancer consists mostly of the ignorant but includes some very competent men. As has been frequently pointed out, there are nearly as many different cancer parasites as people who have claimed the discovery. Some claims are so grotesque as not to be worth consideration, others have been abandoned by their authors. It is probably not going too far to state that, at the present time, no trained and competent observer believes in any particular parasite except the one he has himself discovered—which limits the supporters of parasites to one man for each parasite.

It is necessary here only to consider the general grounds of disbelief in any specific micro-organism as the cause of cancer. Of course, it is not possible to take a definite stand and say that cancer cannot be due to an organism but that it can be so caused is eminently improbable.

As I shall show later, malignant growths may sometimes be transferred from one individual to another by grafting small portions of the tumour; but in no case will the tumour cells survive in an animal of another species or even of another variety of the same species. We know of no parasitic micro-organism in mammals of which this is true. In the case of "wheat rusts," one or two varieties of wheat may be susceptible to a particular variety of rust but all other kinds of wheat are immune to this particular variety of parasite. Thus parasite X may thrive on variety A of wheat but wheat B may be naturally immune. There is a way, however, by which X may be rendered capable of attacking B. Parasite X is able to live in another variety of wheat C; if it be allowed to live for some time on C, it is found to be capable subsequently of living

¹ James Ewing, *Archives of Internal Medicine*, vol. i. 1908.

² Borrel, *Bull. de l'Inst. Pasteur*, 1907, v. 497, 545, 593, 641.

on A. C is called the bridging species. Somewhat similar attempts have been made in the case of cancer. Growths originating in one breed of mice have been transferred with difficulty from race to race (*e.g.* English, French, German, Danish) but never survived when subsequently introduced into rats for a longer period than it did before it had been passed through two or more different races of mice. Consequently, if cancer be caused by a parasite, there must be a different parasite for every different kind of animal that suffers from cancer; none of the parasites must be able to survive in any species or variety of animal except the one to which it belongs: yet all these different parasites produce precisely the same results in the different kinds of animals. All the parasites which we know to be capable of causing the same disease in different kinds of mammals are able to survive in a number of different species.

But this after all is one of the lesser difficulties in accepting a parasite as the cause of cancer. Many of the points involved in some of these difficulties are so technical that short of writing a treatise on the general pathology of tumours, it would be impossible to make them clear to the general reader. One or two of the most striking examples must suffice.

Having gained an entrance to the system, though in some cases parasitic micro-organisms may remain more or less localised, when they extend their ravages upon their host to different parts of the body they produce similar changes in the cells and similar results whatever may be the tissue they attack. Some parasites show a preference for particular parts of the body or particular kinds of tissue, others do not. Malignant growths occur in every tissue in the body with but few exceptions, such as nervous tissue. When, however, a metastasis, that is a secondary tumour or extension of the disease to another part of the body, occurs in a person suffering from cancer, this metastasis consists of cells similar in character to the original or primary growth: it therefore must be supposed that when the parasite gains entrance to the body of an animal, it takes on a new power which enables it, when it passes to another part of the body of its host, to transform the cells of this other part and give them the characters of the cells among which it lived at first in the body of this particular host. The only other alternative is to believe that besides a different species of cancer parasite existing for

every species of animal subject to cancer, there is also a different parasite for each of the many different kinds of malignant growths. The different kinds of malignant growths found in man are found also in other animals. For instance, cancer of a gland is similar and has similar varieties in mice and men, both microscopically and in general behaviour. It would therefore be necessary to assume that the widely divergent varieties of the cancer parasite in man have representatives in the independent groups of parasites belonging to each variety of animal.

Cancer of the uterus may arise during pregnancy but the disease is not transferred to the offspring; *vice versa*, a child may be born with malignant disease but the mother will be free from it. This does not appear to be compatible with a parasite which has such free powers of migration as a parasite causing malignant growths must necessarily possess.

There are some parasites known to cause specific diseases, which may also be among the causes of cancer. The parasite of syphilis is an example. But to say this is not to suggest that the parasite of syphilis or any other parasite is the cause of cancer. That diseases and conditions producing chronic irritation and inflammation and consequently an unusual multiplication of the cells of a particular area should cause some of the cells to pass out of somatic co-ordination and thus originate a malignant growth, seems to be in every way in accordance with what we know of cancer. A specific parasite is in no way required in framing an adequate explanation and the difficulties in the way of conceiving a micro-organism to be possessed of the necessary qualities appear to be insuperable. The theory of somatic co-ordination or cell autonomy, as set forth in the last number of *SCIENCE PROGRESS*, though affording a poorer prospect of a speedy discovery of a cure, is compatible with all the known facts. The conception of a parasite has been carried so far, however, that a process has been described by which certain bacteria multiply either in the body of the host or in artificial cultures in such a way that exact representations are produced of the minute structure of the individual cells and of the arrangement of the groups of cells found in different kinds of tissue.¹ The author of the account certainly does not say

¹ Marie Bra, *Culture in Vitro des Cellules Cancéreuses* (Paris: A. Poinat, 11, Rue Dupuytren, 1909).

whether or not, when a group of these bacteria has multiplied beyond the limit necessary to the imitation of one cell, the process of cell division (mitosis) is imitated as the image of a second cell is formed, which would be necessary as mitoses are particularly numerous in many cancers. It is difficult to see how evolution or any other process could have brought about a case of mimicry which could only have been observed by the individuals attacked by the organism since the invention of the modern microscope. Mimicry which protects or otherwise benefits the mimic can be understood. Mimicry such as this is inconceivable. Various micro-organisms are frequently found in cancer but these are found also in other diseased conditions and they are not always present in cancer.

EXPERIMENTAL INVESTIGATIONS

During the past ten years a very large number of experiments have been carried out with carcinomata occurring in mice. One reason for this has been that some of these tumours have been found to be transmissible—that is to say, on transplantation from one mouse to another they grow in the new hosts in a variable proportion of cases, the proportion of successful transplantations being dependent upon several different conditions.

An impression seems to exist that the present activity in this particular branch of experimental work followed immediately upon the discovery that tumours could be transplanted. This is not a correct impression. The activity is due to the fact that public interest in cancer research took a practical turn about ten or twelve years ago and that means were provided for experimental work.

The first successful attempt to transplant a malignant tumour from one individual to another appears to have been that made by Novinsky, who transferred a cancer occurring in the nose of a dog into two other dogs.¹ He was followed by Wehr² and by Hanau,³ the former transplanting a sarcoma occurring in a dog, the latter an epithelioma occurring in a rat. Morau,⁴ several years later, successfully transplanted a carcinoma in

¹ *Centralbl. f. d. Med. Wissensch.* Berl. 1876, xiv. 790.

² *Arch. f. klin. Chir.* Berl. 1889, xxxix. 226.

³ *Ibid.* 1889, xxxix. 678.

⁴ *Arch. de Méd. Expér. et d'Anat. Path.* Paris, 1894, vi. 677.

mice and since then the number of successful transplantations has been enormous. The obvious advantages of using so small and cheap an animal adequately account for its popularity for experimental purposes. Whether malignant growths are really more common among mice than other mammals, as has been suggested, is very doubtful. In the case of no other animal have hundreds of thousands, perhaps millions, been kept for the particular purpose of making observations upon cancer and for breeding experiments. All that has been suggested by the facts is that cancer is nearly as common as it is in human beings and that, therefore, it may also be common in other mammals, though we have no data at present upon which to base a definite statement.

Some important points with regard to cancer have been established by these experiments. Cancer is transmissible from individual to individual but only through the transference of the *living cells* of the growth from the individual in which they originate to a suitable position in the body of another individual. The cells of the growth, though they may live and multiply for some time in a closely related animal,¹ are only to be established in an animal of the same variety of the same species and the more nearly the animals are related to each other, that is to say, the nearer their common ancestry, the greater will be the percentage of successful graftings.² It is certain that these transplantation tumours grow from the transplanted cells and not from the cells of the new host.³

Successive generations of tumour, that is to say, successive sojourns in fresh individuals as hosts, if the hosts are of the same near ancestry, increases the percentage of successful grafting. The rapid passage through successive hosts increases the rapidity of the growth of the tumour.⁴

With regard to the experiments demonstrating this latter

¹ Ehrlich, *Arb. a. d. k. Inst. f. exp. Therap. zu Frankfurt a/M.*, Jena, 1905, i. 77; Apolant, *Therap. der Gegenwart. Berlin u. Wien*, 1906, xlvii. 145; and many others subsequently.

² Jensen, *Central. f. Bakteriöl. u. Parasit*, Jena, 1903, xxxiv. 122; Haaland, *Berl. klin. Wochenschr.* 1907, xlv. 713; and many others.

³ Jensen, *op. cit.*; Loeb, *Journ. Med. Research*, Boston, 1901, vi. 28; and very many others.

⁴ Ehrlich and Apolant, "Beobachtungen über maligne Mausetumoren," *Berl. klin. Woch.* 25, 1905, and *ibid.* "Experimentelle Beiträge zur Geschwulstlehre," 6 1906.

fact, the authors say that the results were due to the carrying out of a definite plan, using a great number of animals and transplanting as rapidly as possible. "Our object was to increase the malignancy of the tumour cells to the maximum by the continued systematic passage from animal to animal according to the analogy of bacteriological technique."¹ Whether another interpretation of these results is not more probable will be considered later.

Bashford, Murray and Bowen² have observed alternations or waves in the rate of growth and viability involving several generations of the transplanted tumours with which they have worked and they interpret this as being due to a rhythm in the growth energy. Calkins records similar waves³ but concludes that they are due to some cause within the cancer cell itself and considers that this cause is probably an intracellular parasite such as *Plasmodiophora brassicæ*. Apart from other considerations which make it almost impossible to accept a parasite as the probable cause of cancer, Calkins' paper shows such intrinsic signs of carelessness that the observations described in it cannot be taken as bearing much weight. Another interpretation of the significance of these waves of growth will be suggested shortly.

A general impression conveyed by a consideration of the literature dealing with experiments upon these graftable mouse cancers is that they differ to a large extent from primary malignant growths occurring in the human subject. Metastases or secondary growths are very rare. When they have been described, they have generally followed only upon inoculation with an emulsion of tumour cells and not upon the grafting of a solid piece of tumour tissue. It must be obvious that the former method is one that enables single cells to gain access to a small blood-vessel and be carried to the lungs, where, if they survive, a tumour will develop but only become noticeable later than that formed at the site of inoculation. It is also almost certain that when the emulsion is injected forcibly under the skin or into the peritoneal cavity, isolated cells or groups of two or

¹ It has been demonstrated that a strain of certain disease-producing micro-organisms may be rendered far more virulent by a rapid succession of inoculations from animal to animal.

² *Proc. Roy. Soc.* 1906, B. lxxviii.

³ *Journ. Exper. Med.* vol. x. 3, 1908.

three cells must often be driven further from the bulk, be scattered, in fact, and give rise to smaller tumours which become noticeable later than the main tumour. It is remarkable that practically all these secondary growths have been in the lungs, though secondary growths in the lungs do not occur in the ordinary course of primary carcinoma. These grafts have always, in my experience, been enclosed in a definite capsule. On following the sequence of events from a few hours after grafting up to fourteen days, at first at intervals of twelve hours and subsequently of twenty-four hours, I found that the inflammatory reaction in the surrounding tissues of the mouse began almost at once: whilst not a single tumour cell which had been introduced into the animal showed any sign of multiplication until twenty-four hours after grafting, the inflammatory products had by this time completely surrounded the graft. Long before the cells of the graft had begun to multiply actively, the inflammatory reaction had already cut them off effectually from the surrounding tissues. The inflammation was always in advance of the proliferation of the tumour cells. This accounts for the rarity of metastases. I have but once personally observed one in the many thousands of inoculated mice I have examined except in those infected with emulsions of tumour cells.

One of the most characteristic features of primary malignant growths is that when they are well established and have reached a considerable size, their removal by operation is almost invariably followed by recurrence. Operations are completely successful only when performed at an early stage. An operation is often desirable in order to prolong life and to avoid unnecessary suffering, when there is practically no chance of a complete cure. With the graftable tumours in mice and rats, however, the case is very different. I have just completed a series of experiments in which I have removed tumours from mice and rats. These were in every instance large and well-established growths, in many cases approaching in size that of the body of the animal from which they were removed.¹ In only eleven cases out of forty-four has the tumour recurred and in these a second operation

¹ In about 80 per cent. of the mice the peritoneum was involved and the operation often included an incision in the peritoneum from the ribs to the pelvis, besides the removal of a considerable portion of it. The two mouse tumours used were of a particularly virulent kind.

has been successful in every case. The recurrences are easily explained through a small portion having been left in the first operation. When a mouse weighs 30 grammes and a tumour has to be dealt with which perhaps is irregular in shape and weighs from 15 to 20 grammes, requiring therefore a considerable amount of dissection to remove it, it is obvious that some of the tumour cells may have been conveyed to the adjacent tissues and left behind or that some outlying portion may have been missed. Operations in rats have always been successful in the first instance. In any case, as the second operation to remove the remainder has invariably been successful, these graftable secondary tumours must be placed in a category different from that in which primary tumours are included.

The method of using emulsions instead of pieces of tumour has been adopted by many observers. Bashford¹ and others have emphasised the need of using accurate doses of tumour cells, stating that only thus can certain errors be eliminated. However desirable accuracy of dosage may be, it cannot possibly be gained by using emulsions of cells, as only living cells are effective. Even in a solid piece of tumour, there must be an unknown number of dead and degenerating cells and many must be killed outright and many more injured in the process of preparing an emulsion. As it must be quite impossible to estimate the proportion of living cells in a measured quantity of emulsion even to within 50 or 75 per cent., I do not propose to touch upon any experiments based upon accuracy of dosage and have only referred to the method as being a possible source of error with regard to the so-called metastases from inoculated tumours.

It is curious that continual contemplation of little else than these transmissible mouse tumours seems frequently to lead to the adoption of methods really untrustworthy and very misleading for which intense accuracy is claimed. This is illustrated by many of the papers dealing with the subject but by nothing more clearly than by the drawings *to scale* of the outlines of tumours in mice at various stages after inoculation given in the Reports of the Imperial Cancer Research Fund. The accuracy of the drawings in connexion with the accuracy of dosage referred to above constitutes one of the most important factors

¹ "Resistance and Susceptibility to Inoculated Cancer," Bashford, Murray and Haaland, 3rd Scientific Report, Imperial Cancer Research Fund, 1908.

in the general conclusions drawn from the experiments. Some of the tumours and even small outgrowths from tumours represented in great detail in these drawings are less in diameter than the thickness of the mouse's skin. When it is realised that even a stocking will alter the relative proportions of a foot, ankle and leg and that the drawings referred to were made from measurements taken through the mouse's skin, the value of the details becomes more than questionable. When also the impossibility of discriminating between minute collections of tumour cells and the inflammatory tissue which is constantly present is taken into consideration, it becomes obvious that the estimation of size and of shape must always include elements of error which vary inversely with the size of the tumour.

In primary cancer in man, a very marked feature is the invasion of the surrounding tissues and the effect upon the general health as the invasion interferes with the functions of the body. This is particularly marked when ulceration and sepsis occur. In the case of grafted mouse tumours the growth does not invade the surrounding tissues, being cut off by the capsule. Even if the surface of the tumour ulcerate and become septic, the mouse does not generally seem to suffer in general health. The septic products, cut off by the capsule, do not seem to be absorbed to the same extent as they are in the case of cancer in the human subject. Even when the tumour grows to a size approaching that of the whole body of the mouse, general health of the mouse frequently does not seem to be affected.

It has been suggested in previous passages that the cells forming a malignant growth, having passed out of somatic co-ordination and living upon the parent organism as parasites, might in a sense be regarded as separate individuals. The occurrence of meiotic phenomena and other considerations were cited in support of this view; most of the experiments just enumerated upon transmissible mouse tumours may be interpreted in a way that emphasises it still further.

Variation, in so far as our knowledge goes, is an intrinsic property of all living matter. Even two cells of the same organ in the same individual are never the same morphologically. But the differences extend beyond morphological features and include potentialities of growth, resistance or susceptibility to stimuli and other non-morphological characters. Moreover as existing

cells vary from each other, so the cells produced by division must vary from the cell that has produced them and from each other. In these inoculation experiments we have therefore two outstanding sets of variable potentialities : those of the individual mice into which the tumour cells are introduced and those of the cells themselves. Theoretically it should be possible to select particular and obvious characters in either the hosts or the tumour cells and with this idea in view I began some experiments in selecting tumour cells which I am still continuing. Though mice breed quickly, it would obviously be a more lengthy, difficult and uncertain process to breed highly resistant and highly susceptible races of mice. I used mice obtained from the same source throughout the first series of experiments and have repeated them with mice from an entirely different source. The procedure was as follows: Twenty mice were grafted at the same time with pieces of tumour of as nearly as possible the same size. When two or three were large enough to use for grafting, twenty more mice were grafted from the largest. The process was carried on through several generations as quickly as possible. On the other hand one of the most slowly growing tumours was chosen at a later date from the original batch of mice and was used to graft another twenty and so on for several generations, selecting always a slowly growing tumour. In this way I modified the rapidity of the growth and produced three strains of tumour which developed at different rates on the average. The differences between the rates of growth were so very great as to be beyond explanation as the result of chance. Selection also accounts for the fact that whilst, when this tumour first came from Prof. Ehrlich's laboratory, I succeeded in only about 30 per cent. of the graftings, the percentage of successes increased in subsequent generations to nearly 100. Working with another breed of mice, I have had precisely the same experience.¹

Other observers who have found that a tumour became more visible after passing through a series of mice of the same breed attribute this change to the acquirement of a power of resistance

¹ The first series of experiments was carried out in the Cancer Research Laboratories in the University of Liverpool with mice bred in Essex. They have been repeated with another breed of mice from Langside, Glasgow. The figures of these experiments will be published shortly, being at present in the hands of the Editors of the *Journal of Pathology and Bacteriology*.

on the part of the tumour cells. A more probable explanation seems to be that only those cells in the original graft that were most resistant to the new environment survived to divide and produce more cells. Of succeeding generations of cells, whether in the same mouse or after having been transferred to another, all which varied towards less resistance degenerated, whilst those that varied towards greater resistance survived to transmit the favourable variation to other cells, which varied in their turn. This process of selection would go on until a race of cells almost entirely resistant to the environment was produced. When the tumour cells are introduced to a new environment in the shape of a new race of mice, the process would be gone through again, unless of course the environment were so unfavourable to begin with that none of the cells was sufficiently resistant to survive. This interpretation seems to account for the fact that Ehrlich and Apolant¹ were able to produce a very rapidly growing tumour by a very quick succession of inoculations—they were obviously obliged to use only the most rapidly growing cells.² It accounts for the fact noted by Jensen, that a well-established tumour gives a higher percentage of successful grafts than a young one.³ It explains why various parts of the same tumour may give different results when grafted⁴ and that though tumour cells will not survive for long in an unsuitable host, some of them survive and multiply when transferred back to a suitable one.⁵ Unconscious selection also accounts for the so-called rhythms of growth in Bashford's and Calkins' experiments.

Bashford⁶ has suggested that another kind of selection accounts for the production of strains of rapidly growing tumours. He says: "In the light of the wide experience gained, it can be asserted that the technique which consists in the employment of large doses of tumour emulsion and rapid *passage* was responsible for the selection of certain primary tumours which survived the procedure and not for the induction of a marked change in their rate of growth." His

¹ *Op. cit.* 1905.

² It of course applies equally to the method of producing a virulent strain of bacteria referred to by these authors. (See previous reference.)

³ Jensen. *op. cit.* 1903.

⁴ Bashford, *Proc. Roy. Soc., B*, vol. lxxviii. 1906.

⁵ Ehrlich, *op. cit.* 1905; Ehrlich and Apolant, *op. cit.* 1905.

⁶ Fourth Scientific Report, Imperial Cancer Research Fund, 1911.

meaning is somewhat obscure but from the context he appears to imply that as some tumours are more malignant than others, the method followed had the effect of selecting the rapidly growing tumours from among other tumours, because the less rapidly growing tumours could not be successfully perpetuated by the method used. The selection he suggests is that of different kinds of primary growths and not of variations among the cells of the same growth. He refers at some length to variations among cancer cells but his remarks appear to apply only to morphological characters. The mode of selection he suggests might apply in a few particular points with regard to some experiments. It is difficult to see how it can apply to most of the experiments referred to here, which appear to be adequately explained by the selection of variations in potentialities occurring among the cells of the tumours and the transmission of these variations in successive generations of cells.

There are records of other observations which I think throw some further light upon the difference between the behaviour of transplanted tumours and that of primary growths from which they are derived. These refer to the changes in the histological characters of the growths from carcinoma to sarcoma and *vice versa* and from a structure similar to that of a primary cancer to that of a benign tumour. Considerable interest was aroused in 1905 by the discovery in Ehrlich's laboratory that in the tenth generation of transplantations of a carcinoma in mice the characters of the tumour had altered to a mixed sarcoma and carcinoma. In the thirteenth generation this became a large spindle-celled sarcoma.¹ A permanent mixed tumour was also produced from the material of four different strains all of which had originally been carcinomata.

The surprise aroused by these observations, however, was somewhat uncalled for, as Loeb² had some years previously recorded the change of a spindle-celled sarcoma occurring in a rat to an endothelioma, a myxoma, alveolar sarcoma and other forms of tumour upon transplantation to other rats. Apolant³ claims to have followed the microscopical changes in the development from carcinoma to sarcoma and describes

¹ Ehrlich, *Arch. v. d. k. Inst. f. exp. Therap. zu Frankfurt a/M.*, Jena, 1905, i. 77.

² *Journ. Med. Research*, Boston, vi. 28, 1901.

³ *Arch. v. d. k. Inst. f. exp. Therap. zu Frankfurt a/M.*, Jena, 1906, ii. 48.

the cells of the sarcoma as being derived from those of the stroma and not from the carcinomatous cells. Subsequently Bashford¹ made similar claims with regard to a similar change from carcinoma to sarcoma with another strain of tumours.

It is not made at all clear by these observers, however, that the carcinomatous cells themselves do not take on the characters of sarcoma, so the real point of their claim—that the sarcoma develops from the stroma and not from the carcinoma cells—remains very doubtful. Apolant² transformed a carcinoma into a benign adenoma by transplanting it into immunised mice.

In considering these observations, one realises that besides the general effect upon the health of the animal and the other points of difference already referred to between the transplanted tumours and primary cancer, there appears to be a difference in the general history of the succeeding generations of cells which form the growths. There is, I think, no record of a primary carcinoma changing into a sarcoma or *vice versa*, yet such changes in transplanted tumours were noted directly they were brought under systematic observation.

It is quite clear that the conditions obtaining in a primary cancer must be very different, in so far as the cells forming them are concerned, from the conditions to which the cells of the graft are subjected. The cells of the primary growth are subjected to a minimum of selection by the environment, as they or their immediate ancestors have arisen in the identical environment in which they continue. Moreover, they must act less as foreign bodies towards the surrounding tissues from which they arose than do cells introduced from outside and so do not cause that inflammatory reaction which is so marked a feature in tumours growing from grafts. These considerations suggest an explanation of the invasive nature of the primary growth as compared with the non-invasiveness of those arising from grafts and for the rarity or total absence of true metastases in mice bearing tumours produced by inoculation. The more or less stringent selection of those cells possessing high resistance to a change of environment which is involved in the transference to new hosts is probably also sufficient to account for the other differences.

¹ *Berl. klin. Wochenschr.* 1907, xliv. 1238.

² *München Med. Wochenschr.* 1907, liv. 1720.

The cells forming the tumours produced after a long succession of graftings must possess some characters that were not at all necessary to those forming the primary growth; they are able to resist a strange environment and the reaction on the part of the cells of the host which does not exist at all or only in a very slight degree in the case of a primary cancer; they go on multiplying during periods several times as long as the period of life normal in the species of animal in which the primary tumour originated from which they were obtained; and the cells produced after a number of sojourns in strange hosts, involving a number of cell generations many times greater than could possibly have occurred had they remained in the original host, sometimes exhibit very striking and obvious morphological differences from the cells of the original primary tumour. In this connexion it is well to bear in mind the facts relating to the general potentiality of differentiation retained by the cells of the soma.

It seems probable, from a theoretical point of view, that the form of selection to which the cells are subjected in strains of transmissible tumours must tend to preserve those in which the potentiality for independent existence is greatest: that the greater the number of cell generations produced away from the environment in which the ancestral malignant cells arose and the more numerous the different environments through which the descendants have passed, the more similar their characteristics should be to independent organisms. This theoretical probability seems to be borne out by observed facts.

THE PLANET MARS

By JAMES H. WORTHINGTON

BEFORE entering upon the subject of this article, it is advisable that I should state in a few words why it has been written and precisely how the information which it contains was obtained. Being much impressed by what I had read of the Martian features, as detected and portrayed by Lowell and Schiaparelli, I determined to avail myself of the first opportunity, if possible, to see for myself whether or no these features were real, because they seemed to be too wonderful to be believed at second hand. The opportunity came in 1909. Thanks to Lowell's hospitality and kindness, I was able to study the planet at Flagstaff during the opposition of that year and was fortunate enough to see many of the canals and oases and to assure myself of their reality. On returning to Europe in 1910, I found much scepticism prevailing which I scarcely knew enough to refute. I therefore attempted and partially succeeded in seeing the canals again at Nice. This was in 1911.

When the planet again approached opposition, I gladly accepted Lowell's invitation to see more at Flagstaff and accordingly spent two months there, observing the canals and studying them in greater detail. I was able to confirm Lowell's observations and by discussion with him to remove from my mind many obstacles which stood in the way of accepting not only the discoveries but also the explanations which he has put forward.

Having had freedom to travel, I have been able, owing to the courtesy shown to me by many astronomers on my journeys, to study, with the aid of exceptional facilities, the effects of climate upon the astronomical work—a factor the enormous importance of which can scarcely be realised by those whose experience is confined to a single country or even continent.

It seems to me therefore that I may be able to add a few words of interest to the great mass of accounts which have appeared recently upon this most engrossing subject.

From the earth no celestial body is more accessible to observation than Mars, the moon alone excepted. To this proximity is due, in large measure, the exceptional success which has rewarded our study.

At the outset of this inquiry it should be remembered that in space all positions are unique both in their conditions and opportunities. It is therefore necessary, as far as possible, to free our minds from the prejudices which are due to our position and to study the details which have been revealed to us with dispassionate coolness.

It being in the nature of man to seek his likeness, he seeks it before all else, forgetting that when dealing with another planet the one thing which is *a priori* probable is that he will find much that is quite different and so he comes to consider strangeness as one of the hall marks of truth in his discoveries.

Geomorphic ideas have led men into many errors. The so-called seas of the moon have turned out to be the driest of land and the greenish areas on Mars, at first so confidently dubbed oceans, in the light of further research, appear not to be fluid at all.

Thus are we taught to expect the unexpected, and to feel no surprise when three centuries of patient study are rewarded by its discovery in Mars.

With the invention of the telescope came the discovery of the nature of the planets as comparatively cool bodies reflecting to us the light of the sun—a discovery which was announced in the famous anagram of Galileo :

Cynthiæ figuras æmulatur mater amorum.

(The mother of loves [Venus] imitates the phases of the moon.)

In later days his most distinguished compatriot Schiaparelli might well have used his predecessor's words with equal aptitude to express the result of recent work on Mars :

Hæc immatura a me jam frustra leguntur.

(As yet I seek in vain to read the meaning of these incomplete observations.)

It fell to Galileo in the end to expound his epoch-making discovery. The same justification came to Schiaparelli, for though his eyes failed him, he lived to see through those of his successors the confirmation, extension and interpretation of his work.

Soon after the discovery of the disc of Mars, came the announcement from Huygens that the disc possessed surface features from observation of which he felt assured that, like the earth, the planet rotated upon an axis. The marking which revealed this fact is the now well-known dusky wedge called the Syrtis Major.

A little later increased telescopic power showed to the old observers the white areas covering the poles of the planet whose behaviour has turned out to be the master key to the explanation of almost all the detail on the disc which subsequent scrutiny has revealed.

But space does not permit me to follow historically all the steps by which we have acquired our present knowledge of the planet. Sufficient has been said to show that it has advanced *pari passu* with the power of optical instruments.

The investigators who preceded Schiaparelli laid the foundations of areography, as the subject is named which describes the configuration of the Martian surface features—patches of colour, green and ochre, white and grey, which cover the disc with their varied hues, making it appear like a gigantic gleaming opal. On looking at Mars we perceive them at once. Their outlines are well defined and have long since been laid down in maps of the planet.

The delineation of these features was well-nigh complete when Schiaparelli began his studies of the planet in 1877. The opportunity then afforded was an exceptionally favourable one, the planet being very near the earth when showing the fully illumined face of opposition.

At this time the disc was so much dilated by its proximity that with a magnifying power of only eighty diameters it appeared in the telescope as big as that of the moon seen by the unaided eye. Schiaparelli and the world alike were startled on this occasion by the discovery of numerous dark lines criss-crossing in the most unexpected fashion the ochre-coloured regions of the planet.

Following the well-worn analogy of his predecessors—of land and sea areas on the planet—he christened these new features “canali” or channels, which reckless translators at once dubbed canals, a name implying more than the astronomer had actually found on the planet.

At each subsequent opposition he succeeded in seeing them

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again—and seeing them better with growing experience, he added to their number and complexity the fact that many of them consisted of doublets the two component lines of which were rigidly parallel.

Those who could not see the “canali” at all very naturally refused to give credence to them and began to suspect that they were the illusions of their discoverer.

As first seen by Schiaparelli, they were not by any means very regular but as his powers of discrimination increased with practice, he perceived more and more clearly their linear and geometric configuration.

To see these markings at all implies a very great advance in the observer's art, as is proved by the fact that even to this day, though their existence is no longer questionable or questioned, there are few observers who have seen them as well as did their discoverer more than thirty years ago.

The object of this article being to present concisely an account of our present knowledge of the planet, we shall do well to proceed at once to study the methods used by Lowell—Schiaparelli's greatest successor—and the results which he has obtained. Lowell has added more to our knowledge of the planet than the sum total of all that we previously possessed.

At his observatory the mathematical appearance of the “canali” has been confirmed and the discovery of an equally amazing and correlated system of spots—which he calls oases—has been added.

Another advance was made by the detection in the green areas of the uninterrupted continuance of the network of the “canali,” thus showing them to be limited in extent only by the surface of the planet on which they occur.

In order to appreciate the weight of conviction which these discoveries carry, it is necessary to enter somewhat minutely into the means and methods by which they have been achieved. I shall therefore describe them as best I may.

It is often thought, by those unfamiliar with planetary observations, that the larger the telescope the more detail it should reveal; the first step therefore will be to remove this cardinal misconception by a careful consideration of the optical principles involved in the scrutiny of detail upon a planetary disc.

The problem may be succinctly stated as follows: Given a

planetary disc, brilliantly illuminated as in Mars : required, the aperture and magnifying power which will best reveal fine detail upon its surface. It is necessary to digress at once to inquire what happens when we turn the telescope upon a star.

The star disc seen in the telescope is a diffraction effect produced by the lens. It is sufficient for the present purpose to recall the fact that the larger the aperture of the lens, the smaller is this diffraction disc ; but besides the disc there are concentric rings surrounding it arranged in order of brightness, the faintest visible being the outermost.

Now let us suppose that we wish to separate two bright stars which are very close together. In a large telescope they appear perhaps as two discs either in contact or overlapping with their respective systems of diffraction rings interlacing. The confusion apparent to the eye in this picture is further increased by any unsteadiness in the air between us and the star, which causes the two images to swim and flicker ; the rings break and mingle, so that the observer is unable to see anything clearly, the stars appearing as a single pool of boiling light.

The nature of the movements of the air must therefore be considered. These consist of a series of ripples or waves passing across the field of view, whose size may be estimated from the nature of the disturbance they produce. An analogy may illustrate the point.

Any one who has been out in a boat has seen the sea bottom in the shallows on a calm day and noticed how the small objects on the bottom—shells and stones—appear to swing about below on account of the waves. This swaying does not disturb the outlines of the small objects that are visible but merely produces a general rhythmic motion. But if a little breeze ruffle the surface of the water, the minute ripples immediately shatter the image of shells and rock, leaving nothing visible but a confused mass of colour.

Now the analogy between the watery ocean on the earth's surface and the airy ocean above it leads us to expect kindred disturbances ; whether we look down through the one or up through the other, like Newton we may learn something from the pebbles which fringe their mutual margin.

In looking through water—if the attention be confined to a small area—no perceptible distortion of bottom detail is produced by big waves. And so it is with the air also.

Telescopic vision is only concerned with those vibrations which produce disturbance in its field.

Since aerial waves may be of any size up to many yards long, it is obvious that their disturbing effects may be best avoided by the use of a small telescope

In practice it is found that when a telescope three inches in diameter is used these disturbances are generally negligible. Contrasting this small instrument with a three-foot telescope, we see at once how much more we may expect to suffer. If it be assumed that the air waves at the moment are a foot across, then to the smaller instrument they are big waves of which only part of one is in the field at any moment. They will therefore produce general motion but being intrinsically small the motion may well be imperceptible, both on account of its minuteness and extreme rapidity.

The case is very different however in the larger instrument. Here are waves much shorter than the diameter of the lens and since every part of the lens contributes light to form the image there are at the focus the integrated effect of three waves or at least six different phases of disturbance superposed upon one another and producing inextricable confusion.

In this case there is no general motion but instead a continuous blurring of the image. It therefore appears that since air disturbance is inevitable it is best to seek that which is longest and that which is least in amplitude. If the wave be very big, it will produce only an occasional swaying motion of the image which in no way disturbs the integrity of its parts.

We are now in a position to remove the first difficulty there is in viewing the supposed double star—by stopping down the telescope until the image is free from blurring and subject only to general motion.

We accordingly stop down the telescope and the star now presents the appearance of a peaceful, oblong patch of light, somewhat fainter it is true and perhaps a little bigger but something which will give our eye a chance.

The stars are not yet separate. The observer is still balked of his aim—by reducing the aperture he has increased the star discs, which now overlap the more and he seems to be in the quandary of Alice in Wonderland when she had reduced herself with the aid of the magic cake so as to get through the little door in the passage and found that she could not then reach up

to take the key off the glass table. She saw it clearly through the glass, high above her diminished head. But Alice was not at the end of her resources, nor is the astronomer. Alice reduced herself still further and crept under the door and he may further reduce the light of the stars and so see between them. This time he uses a dark glass, the action of which is at once apparent when the nature of the images is considered.

They are brightest at the centre and surrounded by fainter interlacing rings which can well be dispensed with. The tinted glass at once cuts off the light of the rings. It also dims the central image equally all over so that only the brightest part in the middle remains visible. The two middle points of the star images are now seen neatly separated by the gap which previously was filled with the light of their outer edges. So the observer has achieved his purpose in an unexpected way by reducing the light instead of increasing it.

This digression may appear at first sight to have little to do with Mars but it is not irrelevant, for in the telescope the disc of the planet is made up of an indefinite number of luminous points each behaving in exactly the same way as the two star discs first investigated. It is therefore easily seen that the same methods must be used in separating the several points upon his surface.

One might at first suppose that the process might be continued indefinitely. But a limitation is set by the apparent brilliance of the surface, because to see clearly the eye requires a certain minimum of illumination; above this minimum the method may be applied whose importance has long been unaccountably overlooked by many observers.

In the light of these facts it is easy to see that aperture plays at best a secondary part in planetary observation, which is restricted by the climatic difficulties by which we are so greatly hampered on our earth.

Experience in many observatories has convinced me that as yet there is not one which is so highly favoured in a matter of climate as that of Lowell at Flagstaff, Arizona. At this station (at an altitude of a mile and a half above sea level), not only is the air very steady and clear but there is actually less of it and that only the best part left over the observer's head.

Here is then the best place to determine the limits of useful aperture in planetary observation and the result to which

observers have been led here is both instructive and startling, as they have found that, even under conditions so good as to be incredible to those who have not seen them, no advantage in definition is gained by dilating the aperture beyond eighteen inches; and when the conditions are less than the best, a very perceptible loss of detail occurs.

It seems probable that until some better climate be found, no very substantial advance can be made in the effective power of our instruments but as yet so little is known of the conditions prevailing in out-of-the-way localities that it is quite likely that diligent search may reveal a better place. Meanwhile we must console ourselves with the knowledge that the optician has done all he can for the problem, having made telescopes much larger than the astronomer can use profitably.

Having made this discovery, we must turn our thoughts from the lens at the big end of the telescope to the man at the small end, whose qualifications must now be examined.

Only those whose profession is the use of their eyes can realise how much training is both necessary and possible and how much the degree of proficiency attained depends upon the nature of the training. Just as musicians are called upon to learn different instruments, so astronomers are called upon to view different objects.

There are two main divisions of visual astronomy—stellar and planetary—differing from each other in as many essentials as do fiddling and piano playing. In the case of a star, the observer knows what he is seeking—namely a small disc of light; all he needs is to see that the star is there.

The case of a planet is different. The disc is there, it cannot escape notice but we are not concerned with it but with its parts. The glimpses of detail which our troubled atmosphere permits us to obtain are but momentary and therefore one of the first essentials is that the observer shall cultivate quickness of perception as well as acuteness in discrimination. Herein lies the fundamental difficulty of Martian observation which only long practice can surmount.

When the conditions are not the best, only the very quick observer will be able to see anything properly. The canals may flash into sight repeatedly without the inexperienced observer ever perceiving them. He must wait for one of those rare occasions when the detail is steadily visible during a second or two, in

order to be assured of its reality. He will thus find out what to seek and believe. It is an old story. To be discovered, a fact must force an entrance into the stronghold of men's minds; when once it has achieved this it becomes a welcome guest.

This fact has been already exemplified in the case of Mars. His satellites required a twenty-six inch telescope and persistent care for their discovery but have often since been seen with telescopes of less than half this size.

Although the more salient details of the disc of Mars may be corroborated by any observer who has the needful practice and patience, the discrimination and discovery of the more intricate and minute parts require special qualifications which few possess and practice cannot give them. I refer to the intrinsic defining power of the observer's eye considered as an instrument.

Lowell has pointed out that there are two useful extremes in eyesight which cannot meet—defining power and sensitiveness to light. Suitable education of the eye assists by drawing the two extremes nearer together but the possession of either quality in a superlative degree excludes the other.

In the retina on which the image falls there is a structure of rods and cones varying markedly in size and texture in different eyes. Those having the finer texture have also the greater defining power but are deficient in sensitiveness. A photographic analogy may help. Rapid plates are more sensitive to light and of coarser grain than the slower plates which give a sharper picture. The increased definition on the slower plate is due to the fact that the finer grain produces less distortion of the detail which falls upon it.

To return to Mars. We find at once among observers of the planet a striking contrast. Prof. Barnard, who by his discovery of the fifth satellite of Jupiter (an object of excessive faintness) proved the sensitiveness of his eye, finds himself entirely unable to detect any of the "canali" which are so evident to Lowell.

Of course some of this discrepancy is due doubtless to differences of climate and instrument but there remains a residuum which can only be explained by a difference of eyesight. Fortunately for the elucidation of the problem many—like the writer—possess eyes intermediate between these two extremes, so that to some extent they may share the discoveries

of both. Of this I may perhaps be permitted to quote an instance.

Searching for canals at Flagstaff during the opposition of 1909, using a yellow screen before the eye-piece and an aperture of 18 in., I was amazed, on glancing off the disc to the surrounding sky, to see a minute point of light, which turned out to be one of the satellites Lowell, when his attention was drawn to it, perceived it also. Canals were visible to him which I could not see and the satellite which had escaped his notice was evident to me.

There are many features visible on Mars which can only be represented by drawings and to make these successfully requires special qualifications of memory in the observer as well as quick and acute vision. To be convinced on this point it is only necessary to read the reports on the recent eclipse of the sun, a phenomenon so fleeting as to serve our purpose well.

As many readers may remember, this eclipse was just total on the central line in Portugal during perhaps a second, certainly not much more. I quote from an observer who was very near this central line. Referring to the orientation of the solar crescent in mid-eclipse he says: "In the excitement of the moment I did not see whether the crescent of the sun as it passed from the left to the right side of the moon passed below or above it." Again he says: "As the event proved we were too far south-east to be in the track of totality."

It is certain from his position that the crescent did pass on one side only of the lunar disc. Further it is clear that the passage of the crescent must have been comparatively slow, occupying at least a large fraction of a second. Also the observer was not without experience, as he was observing a total eclipse for the fourth time. It is therefore evident that the omission which he so honestly admits was not one of eyesight but of memory.

As has been said, the best views of Martian detail seldom last a second. The positioning of this detail is of the same order of difficulty as the observation quoted.

The next point which claims the attention of the observer is his skill, which means command over the materials which he uses. Many misconceptions of the appearance of Mars

have arisen from the extreme difficulty of drawing the delicate detail that is seen. We have only to look at various drawings by different observers to be assured of this. Comparing the drawings, it is difficult to believe them to be *bona fide* attempts to portray the same object.

Lowell tells me that after twenty years of practice in this particular work, he is quite unable to draw the canals of Mars as they appear in the telescope. His practised hand cannot trace lines on paper fine enough or straight enough to represent them. It is therefore natural that the attempts of less experienced observers should be but caricatures of the planet which they strive to represent. It is, however, a relief that the drawings made independently at Flagstaff do resemble one another and the planet very closely, thus affording internal evidence both of the reality of the features seen and the accuracy of the representations.

Turning now to the method by which detail is detected, we find that the process, unlike the announcement of the discovery, is not a sudden one. Let us follow the observer to the dome and trace his method. Armed with a suitable dark glass and an appropriate aperture, as explained earlier, he watches the planet carefully. Suddenly he is startled by the appearance of some previously unknown marking which flashes into sight but for a moment and is gone, leaving only a vague impression of something being there. The hint so obtained must be noted, for perhaps, later on, another and another glimpse may be obtained which by their cumulative effects assure us of the reality of the new feature.

This is the manner in which all the canals have been discovered and just as accumulated observations establish their numbers, so accumulated hints attest the existence of the fainter markings, until a moment of perfect seeing shows them in all their beauty with the fineness and fixity of a steel engraving.

At first sight their elusiveness suggests an illusion, which accordingly claims our attention next. Optical illusions may be divided into two classes—those which are self-confessed and obvious; and those specious appearances of reality which may deceive all but the most penetrating analysis.

As an illustration of the harmless class of illusion, irradiation

may be taken, which is the apparent enlargement of a bright disc when seen against a dark background. By trial of the different contrast effects to which this phenomenon is due, its laws may be determined and its effect eliminated from observations which it might otherwise vitiate.

An instance of the deceptive illusion is the often-quoted power of the eye of integrating minute markings too small to be severally visible. On looking at a mass of small specks too small to be seen clearly apart, the eye has a strong tendency to accept the specious appearance of these as lines and they cannot be distinguished from realities except by the closest scrutiny. Happily this illusion is only possible under critical circumstances of distance on the narrow borderland between seeing the dots as they are and not seeing any trace of them.

Now the lines which skilled observers have perceived on Mars have been seen under many varied circumstances of distance, illumination and instrument. It seems therefore impossible that they can be due to this form of illusion. Also it is certain that though a series of dots may masquerade as lines, the converse action is inconceivable. Since also dots and lines are visible on Mars at the same time—oases and canals—the assumption of the reality of both seems warranted.

There is another illusion to which the double canals have been, I think erroneously, assigned, namely double vision. Why double vision should be specified I know not, for multiple vision is equally possible. We all know that by imperfectly focussing an object we may, under certain conditions, see it double and if strong contrast occurs we may in the same way induce multiple vision.

Now on Mars are many double canals but illusion suggests that the most conspicuous should be double or multiple. On Mars I know of many cases of faint canals which are double and conspicuous ones that are single but none which are multiple. The canals which appear double appear so from some cause on the planet and not in the eye. They are alike indifferent to and inexplicable by any illusion of the observer's eye and the individuality of the behaviour quite definitely shows. It is the failure to explain the Martian markings as the results of illusion that assures us of their reality.

In this preliminary account I have but summarised the methods and means, the illusions and difficulties which beset

the path of the observer and so paved the way for a description of the detail which patient attention has disclosed; this will be given in a later article.

This preliminary discussion is needful because of the weird oddity and utter strangeness of the features discovered; unless attested by methods of proven accuracy these would be quite incredible and therefore liable to be regarded as the tricks of fancy rather than as the discoveries of painstaking research.

VARIATIONS IN PASTURES

By C. T. GIMINGHAM

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A most important place is taken by pasture and meadow land in British husbandry; indeed, if the area of each crop grown throughout the country be a measure of its relative importance, grass comes before all others. Thus the annual returns of the acreage of land permanently under grass in Great Britain have shown a steady increase during the last sixty years, the area having been enlarged since 1870 from 12,072,856 to 17,446,870 acres, an addition of 5,374,014 acres. In 1911, the returns show that of a total of 32,094,658 acres under crops of all kinds, the area devoted to permanent grass was 2,799,082 acres in excess of that occupied by all other kinds of crop put together. In Ireland, the proportion of grass to arable land is almost exactly two to one; and in some English counties the land is all but entirely occupied by pasture: for example, Somersetshire in 1911 returned 682,342 acres as under grass and only 170,451 acres as arable land. All these figures are exclusive of the rough grass land catalogued as "Mountain and Heath Land used for Grazing" which in Great Britain amounts to another 12,875,660 acres.

Much of the large area referred to is grass land of somewhat inferior quality, this being true especially of the part laid down within recent years. Although some of the heavy clay soils, too expensive to cultivate, in various parts of the country, which were converted into permanent grass land are now excellent pasture, yet most of the land was originally very poor arable and having been allowed to fall down to grass without special care or treatment is at present worth little for grazing purposes. Under proper treatment, a good deal of the poorer pasture land in the country is unquestionably open to considerable improvement; well-planned practical experiments that

have been carefully carried out have already afforded proof that valuable results are to be obtained in this direction.¹

In the present article, however, it is proposed to consider purely scientific soil investigations and it must be admitted that, on the whole, in England, up to the present, the amount of work done on pasture soils and the special problems these afford are not very considerable. All the important contributions to our general knowledge of the factors governing soil fertility have been the result of the study of arable soils, which so far have almost monopolised attention. It is natural that arable soils should have been first studied in detail; but we have to recognise, in applying the results to the case of soils which are permanently occupied by grass, that a number of new conditions are introduced which exert an influence in various directions on the processes going forward in the soil and considerably modify the nature of the problems with which we have to deal. It is most important to know to what extent conclusions based on the study of arable soils are directly applicable to the conditions obtaining in pasture soils and whether the same methods of investigation can be made use of in both cases.

In dealing with grass land, we have primarily to take account of the fact that the soil is occupied by the crop *continuously*. What then is the effect of the long-continued action of one character of growth upon the soil? What differences does the continuous presence of a crop make to a soil from the biological, chemical and physical points of view? There is extremely little detailed knowledge available upon these points and we can still scarcely do more than point out a few of the possibilities and suggest some of the lines along which investigation is still needed.

In the first place, the continuous action of the roots of the same species of plants, always absorbing food and water, always respiring and excreting, by its effect upon the atmosphere within the soil and upon the soil itself, must certainly exert a direct influence upon the nature of the living organisms—and especially of the bacterial flora. In what direction this influence acts can be at present a matter of speculation only: it is possible that it tends to make a more fixed and unvarying flora, one that

¹ See especially the account of experiments on "The Influence on the Production of Mutton of Manures applied to Pasture," by Somerville (Supplement to the *Journal of the Board of Agriculture*, vol. xvii. No. 10).

does not undergo constantly the changes and fluctuations which take place in arable soils. It would seem probable too that a crop which is almost continually requiring food would render impossible any considerable accumulation of readily available plant food in the surface soil, such as takes place under certain conditions in arable soils. In this connexion, it may be noted that we are at present without precise knowledge as to the form in which the pasture grasses take up their nitrogen. Recent work on the assimilation of nitrogen by plants has shown that perhaps many more types of compounds are available as sources of nitrogen than was formerly supposed¹ but almost all the experimental work has been carried out with cereals and leguminous plants. There is definite evidence, however, that ammonium salts, as well as nitrates, can serve directly as food, at all events for some species of pasture grasses. In the case of the grass plots at the Rothamsted Experimental Station which receive heavy dressings of ammonium salts annually, it has been found² that nitrification takes place only to a very slight extent and is probably confined to the immediate neighbourhood of the scattered particles of calcium carbonate present in the soil, since the soil generally is acid. None the less, on these plots a fairly heavy crop of coarse grass is grown, consisting almost entirely of three species—*Holcus lanatus*, *Alopecurus pratensis* and *Arrhenatherum avenaceum*—forming tufts with bare patches of peaty decayed vegetation here and there.

The continual occupation of the land by a crop undoubtedly has a most important influence on the physical condition of the soil. The surface of grass land is disturbed only to a minimum extent and consequently its physical condition and texture are quite different from that of a well-tilled arable field on the same soil. This has far-reaching effects. The undisturbed condition of the surface and consequent slight aeration have a large share in determining what will be the predominant types of bacteria; and one of the evident results of the defect is that those types are favoured which cause the decay of organic matter to proceed much less quickly than in well-aerated soils; and there is always a certain accumulation of humus. This is especially seen in grass land on heavy clay soils and on soils deficient in

¹ See Hutchinson and Miller, *Jour. Agric. Sci.* vol. iv. p. 282, for bibliography.

² Hall, Miller and Gimingham, *Proc. Roy. Soc. B.* 80, 1908.

lime. The importance of such an accumulation of humus in modifying soil texture need not be enlarged upon.

In view of these and many other important factors, certain questions at once arise. For example, does mechanical analysis, which has given such valuable results in the study of arable soils, afford equally useful indications in the case of pasture soils? Is chemical analysis a useful guide? If so, can the large number of data obtained from arable soils be taken as standards? And, to put the whole matter as briefly as possible, how far, in considering pasture soils, must we modify our ideas of the relative importance of the various factors which constitute what may be termed the fertility of the soil?

Such are shortly some of the more general questions. In addition, a large number of local problems of considerable complexity arise in connexion with pasture land and, as has often happened in like cases, the detailed investigation of some of these has served to throw light on the larger problems.

Pasture Soil Analyses.—The value of soil analysis as a guide to the manurial treatment of poor pastures has been dealt with by Wood and Berry¹ of the Cambridge University School of Agriculture, in connexion with a series of experiments on methods of improving poor grazing land; the agricultural results have been discussed by Middleton.² The soils from a number of centres at which the experiments were carried out were examined, in order to ascertain whether the results of the soil analyses could be correlated with the results of the various methods of treatment. Of the latter, the most important was the remarkable improvement effected in almost all cases by the use of basic slag; but determinations of the total phosphate present in the soils gave no indication of deficiency in phosphoric acid. On the other hand, the figure for "available" phosphates (*i.e.* soluble in 1 per cent. citric acid) was of greater value and appeared to be a trustworthy guide as to which soils might be expected to respond to phosphatic manuring, "if for pasture soils the limit below which 'available' phosphate may be considered deficient is fixed as high as 0.02 per cent."

Other results indicated that the figures for total nitrogen, total potash and lime were not of much help in determining the best methods of manuring; but if the soil contain not more

¹ *Jour. Agric. Science*, vol. i, p. 114.

² *Ibid.* vol. i, p. 122.

than 0.01 per cent. potash soluble in 1 per cent. citric acid (available), the authors consider an application of potash salts is likely to be useful. With regard to lime, unless a pasture soil contain less than 0.25 per cent. it seems improbable that liming is necessary.

The mechanical composition of the soil is probably the factor of prime importance to take into account in attempting to improve poor pasturage. A fairly good mechanical condition is essential: soils with a very high proportion of either the coarsest or the finest grades of particles are never likely to make really useful grazing land, whatever the manurial treatment.

A further paper by S. F. Armstrong¹ (also from the Cambridge School of Agriculture) deals primarily with the botanical and chemical composition of the herbage of pastures and meadows but includes observations on the soils of the grass lands investigated. It was apparent that, at all events in the English Midlands, the choicest grazing land was invariably associated with soil rich in "available" phosphates; here again the importance of good physical condition and of an abundant supply of "available" phosphoric acid for the production of good pasture land is emphasised.

To what extent these conclusions hold good for pasture soils generally can only be determined when we are in possession of many more data on the subject.

Romney Marsh Soils. — An important local problem has received attention in the very thorough investigation recently carried out by Hall and Russell² of the Rothamsted Experimental Station on the pasture soils of Romney Marsh. Romney Marsh, which has an area of nearly 120 square miles, is part of the large stretch of alluvial land which borders much of the coasts of Kent and Sussex. It is only slightly elevated above high-water mark but having been elaborately drained is now dry and can no longer properly be called a marsh. It is almost entirely grass land. In spring and summer the fields are occupied by great numbers of sheep, as they form some of the best grazing land in the south of England, many of the pastures being famous for their richness. The best land will fatten as many as ten sheep per acre during the summer without the aid of any artificial feeding; but all the pastures

¹ *Jour. Agric. Science*, vol. ii. p. 283.

² *Ibid.*, 1912, vol. iv. No. 4.

are by no means equally good, adjoining fields, in some places, showing extraordinary differences in feeding value. Land is often found surrounding the most valuable fattening fields which can only be used for breeding upon or that will just keep sheep growing. The two types of land are referred to as "fattening" and "non-fattening" pastures; the immediate object of the work undertaken by Hall and Russell was to discover the causes underlying the remarkable differences they exhibit.

Samples of the grass were obtained, at various centres, at different times of the year, from fields representative of both fattening and non-fattening pastures; these were examined botanically and chemically. Samples of each foot of soil down to the water level (usually about eight feet) were also taken and submitted to mechanical and chemical analysis; moreover borings were made to determine the water content of the soil at various depths at different seasons, and during 1909 and 1910 regular observations were made of the water level in the fields and of the temperature at twelve feet and six feet below the surface. By these means it was hoped to detect differences which might lead to an explanation of the obvious differences in feeding value between the two types of pasture.

The investigation of the botanical composition of the herbage from the various fields showed that the most abundant grass was *Lolium perenne*, which formed from one-third to four-fifths of the total herbage on all the pastures; *Agrostis alba* and *vulgaris* were regular constituents up to 20 per cent.; there was also a fair proportion of white clover, though this is not evident in the analyses, owing to the creeping habit of the plant, which made it difficult to include it in the cut samples. The floral type was on the whole remarkably similar in fattening and non-fattening fields. No differences were brought to light by the analyses which could at all account for the higher feeding value of fattening fields.

There were, however, certain differences in the herbage evident to the eye which were not brought out by the botanical analyses. On the good land, the growth of grass was essentially leafy and covered the ground much more effectively than on the inferior land, where a marked tendency to the production of a stemmy herbage with abundant flower-heads was noticeable. This was seen very remarkably in one case, a non-fattening field being covered with the yellow blossoms of buttercups when the

adjoining fatting field showed none, though on analysis of the herbage there proved to be almost exactly the same percentage of the plant in both fields. The characteristic leafiness in the one case and stemminess in the other was the chief difference between the good and bad fields and was quite independent of the floral type.

Three other differences more or less marked were noted—in any pair of fields the good one had more clover in the herbage, showed less tendency to burn in summer and probably gave a slightly higher yield of grass.

The soils next claim attention. Speaking generally, the surface soil in Romney Marsh is of a heavy, close-grained type (though in a few places a lighter soil occurs) and is made up largely of clay derived from the heavy soils of the Lower Wealden strata which has been deposited as silt. The soils differed a good deal at the three selected centres in the Marsh at which the investigations were carried out but as these differences seemed to have no bearing on the present problem they need not detain us. At centre No. 1 (Orgarswick) the soils proved to be very uniform in mechanical composition to a considerable depth and the fatting and non-fatting fields showed no significant differences in this respect. The soil was heavy, containing no coarse sand and about 25 per cent. of the clay fraction; below 7 ft. peat saturated with water was reached. Mechanical analyses of the soils failed to reveal any reason for the superiority of one field over another, poor and rich land being almost identical in composition.

The water content of the soils of both fields at different dates was always practically the same, the fatting field being perhaps a little more moist in early summer and somewhat dryer later on.

There was a small difference in the level of the water, this being always higher in the fatting field. On the whole the soil of the fatting field tended to keep a little dryer and to get rid of its surface water rather more quickly and thoroughly. This may probably be taken as indicating some difference in texture not revealed by mechanical analysis. Differences in soil temperature were very slight but regular, the soil of the good field proving to be a little warmer than the other.

Chemical analyses of the two soils gave very similar results; but a slightly higher percentage of nitrogen and phosphoric

acid (especially the latter) was noted in the good soil. Lime was present in abundance in the subsoil in both cases; and, quoting from the paper under review, "as regards the mechanical and chemical composition, temperature and moisture determinations, little can be found to discriminate between the two soils and though some of the factors of production are slightly better in the good soil the differences seem too small to be significant."

Further examination, however, enabled the authors to account to some extent for the difference in the type of growth of the herbage observable in the fatting and non-fatting fields. The soils of the good fields possess one marked characteristic: they contain definitely more free ammonia and more nitrate in the early part of the season, though the difference disappears later. The accompanying table gives the figures obtained:

NITROGEN AS NITRATE AND AMMONIA IN PARTS PER MILLION OF DRY SOIL, 1910.

		March 16		May 13.		June 22.		September 14.	
		Nitrate.	Ammonia.	Nitrate.	Ammonia.	Nitrate	Ammonia.	Nitrate.	Ammonia.
ORGARSWICK:									
Surface soil.	Fatting .	15'8	11'0	14'6	7'0	1'4	—	9'4	6'0
"	Non-fatting	10'4	11'0	8'9	3'0	3'0	—	58'4	22'0
MIDLEY:									
Surface soil.	Fatting .	12'4	—	24'2	6'5	6'4	—	15'4	—
"	Non-fatting	6'3	—	12'5	2'8	2'7	—	21'7	—
WESTBROKE:									
Surface soil.	Fatting .	23'6	8'0	21'5	20'8	2'3	—	9'4	—
"	Non-fatting	13'0	6'0	7'9	6'0	2'0	—	8'2	—

At the same time it was found that soil from the good field underwent nitrification at a greater rate than that from the bad field. These are the most significant differences observed. It is evident that the better type of grass in the fatting fields is produced as a result of a greater food supply, though the floral type remains the same.

With minor divergencies, all the details given hold equally for the soils of the other two centres, whether the physical conditions of the soils in the pairs of fields were similar or dissimilar. In the authors' words, "... the amount of nitrates and ammonia in the good soils is always far above the quantities found in the bad soils in the early part of the year. Here

is probably the causal factor which accounts both for the greater amount of growth on the fattening fields and its green, broad leafy character. But accepting this more rapid production of available nitrogen as the determining factor giving rise to the herbage, it is still impossible to see from the other determinations made why the formation of available nitrogen compounds should be more rapid in the one case than in the other. The difference apparently lies in the nature of the soil organic matter."

From what has been said, it might be expected that the chemical composition of the herbage from the two kinds of fields would be markedly different especially as regards the percentage of fibre; but this proved not to be the case. The amount of phosphoric acid and nitrogen is slightly higher in the good than in the poor herbage but the difference is by no means enough to account for the contrasts in feeding value. The conclusion is reached that the ordinary methods of food analysis need much refinement in order to give useful results in such cases as this.

Attention should be drawn by this investigation to the very important practical consideration that in dealing with the feeding value of pasture grass it is necessary to distinguish between the effect due to the botanical composition of the herbage (the floral type) and that due to the habit of growth. These are dependent on different sets of conditions. The floral type is more influenced by local climate, situation and management than by soil and may vary considerably on the same field from year to year. The habit of growth in the cases here dealt with appears to be chiefly determined by the supply of nitrates and ammonia in the soil, *i.e.* by the rate of decomposition of the organic matter.

From the point of view of the soil investigator the work indicates forcibly the limitations of the methods of mechanical analysis. Differences in the physical properties and texture of pasture soils exist which are not revealed by mechanical analysis and, indeed, speaking widely, "soil analysis does not give as clear indications with pasture soils as it does with arable soils." This conclusion receives confirmation in some work now to be referred to.

The Scouring¹ Lands of Somerset.—A somewhat similar

¹ Scouring is the farmer's word for acute diarrhoea in cattle.

problem has recently been gone into anew by the present writer.¹ This concerns a considerable area of pasture land in the middle of Somersetshire in a district given up almost entirely to dairy farming. Here again are marked differences in the feeding value of closely adjoining pastures; but in this case the bad fields are actually injurious. In these particular districts the herbage of much of the grazing land has the property of causing cattle feeding there to be scoured very seriously indeed at certain times of the year, such pastures being known locally as "teart" or "turt" land. Their presence naturally lowers the value of the farms on which they are situated, though the extent to which the scouring properties of the herbage are developed varies greatly in different places; and good and bad fields are often intermixed in a very intricate manner. Cows in milk suffer most severely but all kinds of cattle may be affected; lambs also are scoured badly, whilst sheep and horses are, for the most part, exempt. The scouring is usually most prevalent in the autumn—when cattle are feeding on the aftermath—and as a rule the more abundant the growth the more serious the trouble becomes, varying with the season. Individual animals vary greatly in the degree to which they may be affected.

Such scouring on the "teart" lands has been attributed to a variety of causes, among them the presence of some particular plant in the herbage and a bad water supply. Neither of these explanations, however, can be substantiated.

Nor does the trouble suggest a specific disease and attempts to isolate a responsible organism have proved abortive. Infection never travels from a "teart" field to a neighbouring sound field even though only a ditch may separate the two; nor do cattle transferred from "teart" to sound pastures ever bring infection to healthy cattle with which they may come in contact. The usual result of the application of manures to "teart" pastures is to make matters worse as the growth is increased and when large numbers of sheep are fed in these fields the same result is noticed. On the other hand, the first two or three sharp frosts remove all tendency to cause scouring from the autumn herbage.

"Teart" land in Somerset is entirely confined to one geological formation, the Lower Lias. The typical surface soil

¹ Gimingham, *Jour. Board of Agric.* vol. xvii. 1910, p. 529.

on the Lower Lias here is an extremely stiff unyielding clay, blue or yellow lias clay subsoil being not far below. At the same time, a considerable part of this district which lies at a low elevation is covered by an alluvial deposit, varying from a few inches to many feet in depth; pastures on this alluvial soil are invariably free from any tendency to cause scouring even though the typical Lower Lias clay may lie not far below the surface. The division between good and bad land is in many places very sharp and affords an accurate indication of the boundary between lias and alluvium. Where the layer of alluvium is deep the differences in the surface soils of the two kinds of fields are very obvious; and even where the soils are in most respects very similar the surface of the good fields is noticeably darker in colour and looser in texture and, further, the ground has an indefinably different and more springy "feel" to the foot.

In order further to investigate this difference in texture between sound and "teart" land, determinations of the densities¹ of some of the soils *in situ* were made. With few exceptions the surface soils of the sound fields show consistently a definitely lower density than those of "teart" fields from the same neighbourhoods. The sound soils have also a greater capacity for holding moisture.

Ordinary chemical analysis of both types of soil has not revealed anything that could account for the observed effects and there is no obvious peculiarity in the composition of the "teart" fields. A large number of samples were also submitted to mechanical analysis, the result being that all the soils, whether from good or bad land, were found to be of the same general type, so that the observed differences in physical condition and texture cannot be accounted for by referring them to the ultimate mechanical compositions of the soils.

These analyses have, however, brought out the fact that there is, almost invariably, a considerably higher percentage of organic matter in the good soils; and there is no doubt that, to a great extent, the dark colour, the texture and the different appearance and "feel" of the soil of these fields is due to the influence of the higher proportion of organic matter on the nature of the compound soil particles.

The only significant difference discoverable between sound

¹ Details are not yet published.

and "teart" soils lies then in their physical condition. Hence the production of scouring herbage must be determined to a great extent at least by the special texture of the soil. How then does the soil texture affect the physiological properties of the herbage in this manner? Chemical analysis has not so far succeeded in demonstrating the presence of unusual substances in the herbage to which the scouring might be attributed. The ordinary methods of food analysis are not sufficiently refined to detect such differences in feeding value as these; but there must necessarily be a difference somewhere in the proximate constituents of grass from good and from scouring fields. It can only be concluded that under the special soil conditions some abnormal constituent is developed in the herbage having a physiological action provocative of scouring; and further that the texture determines these special soil conditions.

Some evidence is already forthcoming that modifications of the soil texture remove the conditions giving rise to a scouring herbage.

In the two investigations here summarised it is evident that important differences in the textures of the respective types of soils undoubtedly exist which were not indicated by the results of the analysis; and we cannot but draw the conclusion that mechanical analysis according to our present methods is not of the same value in the study of pasture soils as it has proved to be in the case of arable soils. This is probably because of the controlling influence exercised by the organic matter in pasture soils and a place of primary importance must be assigned to this constituent of the soil in determining the type and composition of the herbage. Physical properties which, from analogy with arable soils, might reasonably be inferred from the results of mechanical analysis are, in the case of soils permanently occupied by grass, often masked by the influence of the organic matter present. No doubt the undisturbed condition of the surface soil and the consequent slow rate of decay of the humus accounts for this.

As to the manner in which the action becomes operative we are at present entirely in the dark. Analyses of the herbage by our present methods have led to nothing and it seems probable that before it will be possible to throw much light on this point we shall need to possess more delicate methods

of food analysis and to know much more of the nature and properties of the organic matter as it exists in pasture soils.

[I had an opportunity recently of visiting the Romney Marsh pastures with Dr. Russell and was much struck by the remarkable difference noticeable between adjacent fatting and not-fatting fields. The evidence seems to be all but conclusive that the difference has been induced, in course of time, rather than there was a difference originally between the soils of the two kinds of pasture. It should be noted that both have their value and that both are required, as ewes with their lambs cannot be kept on the fatting fields, the herbage of these affecting the milk and making it in some way deleterious to the lambs. The appearance of the two pastures is strikingly different: the one has the rich, deep green colour characteristic of grass fully provided with nitrogenous manure, whilst the other has the pale appearance of nitrogen-starved grass.

The good graziers are most careful to keep so much stock on the land constantly that the grass is fed off very close to the ground; the sheep are therefore fattened on very young luscious herbage, whilst those on the not-fatting fields doubtless partake of a growth of a more mature character.

There must be a very considerable difference in the composition and food value of the two kinds of herbage. The statement that differences cannot be detected by analysis is merely a confession of impotence—a confession that present methods of analysis are not really of any value—and proof of the need in which we stand of raising the status of agricultural chemistry. Instead of requiring the merest modicum of knowledge, this branch of chemistry is probably one which needs more discriminative power than all the others put together and until we recognise this little progress will be made.

Although the number of species of plants on the two pastures may be the same, it is obvious that there is a far more luxuriant growth of clover on the fatting fields and that the animals on these have more nitrogenous food at their disposal.

Apart from the organic elements, carbon, hydrogen, oxygen and nitrogen, relatively little of value is removed by the fattening sheep from the land; consequently this is constantly and highly manured by their droppings. The sheep with lambs apparently remove more from the soil and return less to it

than the fattening sheep. It is not difficult to understand therefore, bearing in mind that nitrogen is constantly assimilated from the atmosphere by the clover, that the mere excessive usage of the fattening pastures has led to their improvement and that the difference in the contribution made by the two classes of stock to the land may well have brought about the change in the character of the pastures—apparently stock and land have been in reciprocal relationship over a long period of time. The investigation carried out by Hall and Russell appears to be of special value from this point of view: by showing that there is no reason to suppose that the actual soils of the two kinds of pasture differ intrinsically to an extent sufficient to account for the observed peculiarities and that the differences are induced, in all probability, by rational usage, they have in a measure foreshadowed means of improving pastures generally.

H. E. A.]

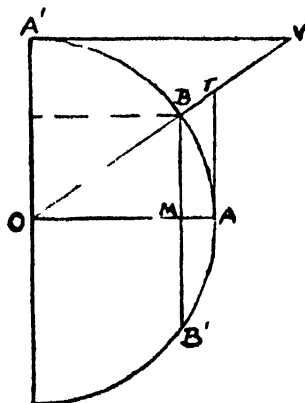
THE GENESIS OF LOGARITHMS

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IN the historical development of mathematics the period covered roughly by the seventeenth century must always, on two counts, be held to be of primary importance, as this century witnessed the birth of the fluxionary calculus and the discovery of logarithms. It is proposed to deal with this latter discovery in the present article. As the details of the discovery are of remarkable interest to the mathematician, and as they are not easily obtained and are couched in archaic language that is somewhat laborious reading, it seems that a restatement of the facts may not be without interest and value.

In order to make the survey fairly complete a preliminary



discussion of the mathematical tables in use at the beginning of the seventeenth century, together with a brief account of the methods of computation adopted, will be given.

Confusion of diction and thought will be avoided if it be remembered that what we call the "trigonometrical ratios" can be and were considered under two aspects—(1) that of ratios, (2) that of lines. In (2), the older system, angles are measured by the arc swept out by a revolving line whose length must be the same for all angles. Then in the figure the line BM measured the sine of the arc AB , OM (the sine

of the complementary arc) was the cosine, OT (which cuts the circle) the secant; and so forth. So that the well-known theorem

$$\sin^2 \theta + \cos^2 \theta = 1$$

would be read by a mediæval geometer as "The square of the sine added to the square of the cosine gives the square of the radius."

It is further to be noted that in the computation of tables, the values of the trigonometrical functions were expressed in integers; so that when additional accuracy was required tables of sines, etc., were computed on the assumption that the "radius" was proportionately large. Thus, in the sixteenth century, Rheticus computed tables for every ten seconds of the first quadrant, taking the radius as 1,000,000,000,000,000; whilst Pitiscus added to this table a few of the first sines computed to the radius 10,000,000,000,000,000,000,000. This manner of presentation is, of course, the mediæval equivalent of our modern phrase, "correct to so many places of decimals."

In more ancient times trigonometrical measurements of angles were based upon the computation of the chord of double the arc, that is to say, in the preceding figure the chord BB' was put in relation to the arc AB . Thus in the first century A.D. Menelaus defines the "nadir" of an arc to be the right line subtending the double of the arc; a table of such arcs and chords was constructed and exhibited by Ptolemy in the second century A.D., in which the chords of various arcs are calculated at intervals of half a degree. The relation of the half-chord to the arc—what we should call the sine of the arc—was known to the Greeks but its familiar use in trigonometry is due to the Hindu mathematicians, whose knowledge, through their Arab pupils, slowly filtered through to the West.

The earliest trigonometric tables of note are those of Johannes Muller, commonly called Regiomontanus (1436-76), who completed an earlier table of sines by Peurbach, in which the radius was taken as 600,000 and the sines computed for every minute of the quadrant. Afterwards, leaving the relics of the sexagesimal notation implied in the above radius, he made a fresh computation of the sines to every minute of the quadrant, taking the radius as 1,000,000. We have also from Regiomontanus a table of tangents—a table which he called

canon fœcundus—computed for every degree and to radius 100,000.

Barely mentioning the canon of sines given by Copernicus, that of tangents by Reinhold (1553) and, about the same date, of secants by Francis Maurolye, Abbot of Messina in Sicily, we come to the striking work of Francis Vieta (1540–1603), without doubt the foremost algebraist of his day.

In a folio volume published at Paris in 1579 he gives tables of sines, tangents and secants for every minute of the quadrant to radius 100,000. And it is to be noted that he regards the “trigonometrical ratios” not as being obtained from a series of lines drawn in or about a circle but as being obtained from a series of plane right-angled triangles in which (1) the hypotenuse has the constant value 100,000, the other two sides being variable and giving the values of the sine and cosine; (2) the base has the constant value 100,000, when the other two variable sides give the tangent and secant; (3) when the perpendicular is kept constant the variable sides give the cotangent and cosecant.

A second table given by Vieta is something of a curiosity, as in it he gives a canon of *accurate* sines, cosines, etc., expressed in integers and rational vulgar fractions. In general, the numbers which express the trigonometrical ratios are irrational but for certain particular angles the values are rational; it is these values which are tabulated. The corresponding angles are not given but in their place appears a series of numbers called by Vieta *numeri primi baseos*. Let one of these numbers be called p and let r be the constant radius which, as before, is taken as 100,000, then, if r be taken as the hypotenuse of the right-angled triangle, the sine or perpendicular will be given by

$$\frac{pr}{\frac{p^2}{4} + 1}$$

the base or cosine will be

$$\frac{\frac{p^2}{4} - 1}{\frac{p^2}{4} + 1} r,$$

with similar expressions for the tangent and secant, cotangent

and cosecant, when r is taken to represent the base and perpendicular respectively of the right-angled triangle.

Such expressions are clearly rational and by giving p , in succession, different values the canon we are discussing was computed.

It should be mentioned that this work, which is of great rarity, was published anonymously. Both Hutton and Montucla agree in ascribing it to Vieta from internal evidence and from the fact that Vieta repeatedly mentions it in his other works.

But perhaps the most massive of all such tables is that computed by George Joachim Rheticus (1514-76), a pupil of Copernicus and professor of mathematics at Wittenburg. The work, which was published in 1596 by Valentine Otho, gives tables of sines, tangents, secants, etc., computed to the radius 10^{10} and for every $10''$ in the quadrant, together with their differences. Theorems and explanations are given for the construction of the canon to the radius 10^{16} and, as in Vieta's work before-mentioned, the trigonometrical ratios are considered as being represented by the sides of right-angled triangles. The computations to the radius 10^{16} , which were made proceeding by steps of $10''$ and for every separate second in the first and last degrees of the quadrant, were published in 1613 by Pitiscus, who added to the canon a few sines calculated to the radius 10^{22} .

With the mention of Lansberg's tables (1591) and Pitiscus's trigonometry (1599), our enumeration of the principal tables in vogue at the beginning of the seventeenth century may be considered to be fairly complete; and now, remembering the absence of all logarithmic aids to computation and considering the large number of significant figures to which the calculations were carried, one can well imagine what a slow, tedious and laborious process was the construction of such tables. Rheticus, indeed, in the compilation of his canon incurred an expense of thousands of gulden, having a large staff of computers continuously employed for a space of twelve years.

The methods used for computation were, of course, very varied: I give here a brief analysis of one process, which will serve as an example.

By the theorems of elementary geometry, the lengths of the sides of a few of the regular figures inscribed in a circle of given radius (10^8 , 10^{10} or whatever figure may be chosen) can

readily be calculated. The angles which these chords or sides subtend at the centre of the circle are also known and clearly half the length of any such chord will give the sine of half the corresponding angle subtended by the chord. Thus in the following table, taking radius as 10⁷, we have

Figure.	Arc.	Chord	Half-arc.	Half chord or sine of half arc.
Triangle . . .	120°	17320508	60°	8660254
Square . . .	90°	14142136	45°	7071068
Pentagon . . .	72°	11755705	36°	5877853
Hexagon . . .	60°	10000000	30°	5000000
Decagon . . .	36°	6180340	18°	3090170
Quindecagon . . .	24°	4158234	12°	2079117

Now, knowing the sine of any angle, the sine of the half-angle can be calculated by means of some such theorem as :

"The sum of the squares of the sine and versed sine equals the square of double the sine of half the arc."

This is, of course, simply the equivalent of

$$\sin^2 \theta + (1 - \cos \theta)^2 = 4 \sin^2 \frac{\theta}{2}$$

and gives $\sin \frac{\theta}{2}$ in terms of $\sin \theta$. And knowing the sine of any angle, we arrive at the sine of the complementary angle by the theorem :

"The square of the sine and the square of the sine of the complement is equal to the square of the radius"; *i.e.* simply

$$\sin^2 \theta + \cos^2 \theta = 1.$$

Starting, then, with the sine of twelve degrees and continually finding the sines of half-arcs, we obtain the following series of tables :

I

Angle.	Sine.	Comp.	Sine.
12°	2079117	78°	9781476
6°	1045285	84°	9945218
3°	523360	87°	9986295
1° 30'	261769	88° 30'	9996573
45'	130896	89° 15'	9999143
22' 30"	65449'4	—	—
11' 15"	32724'8	—	—

Beginning with any of the complementary angles in table I and

continually halving we have, taking, say, 84° and 87° as our starting-points :

II

Angle.	Sine.	Comp.	Sine.
42°	6691306	48°	7431448
21°	3583679	69°	9335804
$10^\circ 30'$	1822355	$79^\circ 30'$	9832549
$5^\circ 15'$	915016	$84^\circ 45'$	9958049
$43^\circ 30'$	6883545	$46^\circ 30'$	7253744
$21^\circ 45'$	3705574	$68^\circ 15'$	9288095
etc.	etc.	etc.	etc.

It is clear that this process can be extended largely by continually taking one of the complementary sines as the starting-point for the halving process ; and thus a large number of the sines may be computed and tabulated.

Returning now to table I, we see that the sines of $22' 30''$ and $11' 15''$ are, to the accuracy needed, in the same ratio as their arcs, and thus sine $1'$ is obtained by simply dividing $32,724.8$ by $11\frac{1}{4}$, giving $2,909$ as a result, and this is, of course, exactly $\frac{1}{48}$ of sine $45'$; so, multiplying $2,909$ by $1, 2, 3$, etc., we have sine $1'$, sine $2'$, etc., up to sine $45'$.

Theorems for the sums and differences of two sines were known and these, combined with the theorems already given for halving, doubling, etc., enabled the calculators to compute any required sine from the knowledge of those given in the preliminary tables and the sines of small arcs.

Usually the sines for the first 30° and last 30° in the quadrant were computed in this way. The remaining gap from 30° to 60° was filled up by using the following theorem :

"The difference between the sines of two arcs that are equally distant from 60° is equal to the sine of half the differences of these arcs." That is, in modern notation

$$\sin(60 + \theta) - \sin(60 - \theta) = \sin \frac{1}{2}(60 + \theta - 60 - \theta) = \sin \theta$$

And hence

$$\sin(60 - \theta) = \sin(60 + \theta) - \sin \theta.$$

Now if θ be less than 30° , $\sin(60 + \theta)$ and $\sin \theta$ are by hypothesis known ; and hence $\sin(60 - \theta)$, which lies between 30° and 60° , is obtained by a simple subtraction.

The canon of sines (and also cosines) being thus completed.

that of the tangents is obtained from the theorem "As cosine is to sine, so is radius to tangent"—the equivalent of $\tan \theta = \frac{\sin \theta}{\cos \theta}$ —whilst, using theorems such as "The secant of an arc is equal to the sum of its tangent and the tangent of half its complement," or "The secant of an arc is equal to the difference between the tangent of that arc and the tangent of the arc added to half its complement," the canon of secants is deduced from that of tangents, by simple additions and subtractions.

Such, then, was the state and efficiency of the trigonometrical tables known to the mathematical world at the beginning of the seventeenth century. The labour involved in such computations as those that we have detailed above as well as the increasing accuracy of astronomical observations gave rise to a demand for a method of calculation which should materially lessen such labours. That method was given to the world by John Napier in the invention of logarithms. These aids to calculation are looked upon as so much a matter of course at the present day and are so strongly associated with "powers," "indices" and what not, that the curious mode in which they originated is apt to be lost sight of. In the writer's view this is unfortunate. Nothing is more common than to hear and read discussions as to whether the modern schoolboy shall be taught to handle logarithmic tables before he is taught the theory of indices and the subsequent deduction of logarithms or not—some holding the latter view, others asserting that to tell a boy he shall not use a table of logarithms until he knows the theory of their construction is as inconsequent as to forbid a boy's using his watch until he knows how to make one of those useful articles. Yet the fact is never brought forward that the discoverer of logarithms had not the ghost of a notion of an index as we know it and that complete tables of logarithms, the direct ancestors of those we use to-day, were printed and in daily use close on a century before the days of Euler, who was one of the first, if not the first, to look upon logarithms as being indices of powers.

Of the life of the discoverer of logarithms few details are known. Born in 1550 and living a life of retirement in a country which was notably wild and lawless even in a lawless age, the antiquary will find little that will help him to reconstruct the daily life of John Napier. A great mass of Napier's

papers perished in a fire which broke out in the house of one of his descendants; in consequence, Mark Napier's monumental life of his ancestor is largely composed, so far as the facts of the life go, of conjecture. What can be rescued from the mass of hypothesis may be briefly condensed as follows: Born, as stated above, in 1550, he was educated at St. Salvator's College, St. Andrews. It is believed that he travelled on the Continent during several years but he was certainly at home again in 1571. Little is known of the details of his home life, save that for years his attention was drawn, like Newton's, to speculative theology. The results of these studies are shown in his treatise, *A Plaine Discovery of the Whole Revelation of St. John*, published in 1593. About this time he seems to have made some progress towards his great discovery, for we are told, on the authority of Kepler, that about this time Tycho Brahe had heard from a Scottish correspondent that a canon or table of such aids to computation was in process of construction. The canon itself was not published until 1614, when it appeared under the title of *Mirifici Logarithmorum Canonis Descriptio*. Napier died in 1617; two years afterwards the posthumous work *Mirifici Canonis Logarithmorum Constructio* was published, which explains the manner in which Napier constructed his canon.

It is a remarkable fact in the history of scientific discovery that Napier's great work sprang, Minerva-like, in full perfection from the head of its discoverer. In the development of the discovery of the infinitesimal calculus, we find all through the seventeenth century foreshadowings in the writings of Cavalieri. Roberval, Barrow and others of the comprehensive calculus finally developed by Newton and Leibniz. But with one solitary exception and that exception as old as the days of Archimedes, we find nothing to show that Napier's discovery was the culmination of a series of stages leading up to that point. The discovery was almost perfectly self-contained.

The exception referred to above is to be found in Archimedes' treatise *Arenarius*, an attempt to extend the cumbrous Greek numerical notation so as to include integral numbers of extremely large magnitude. With the structure of this treatise we need not here concern ourselves. What is important to our purpose is to note that Archimedes incidentally develops therein some properties of geometrical progressions, one of

which contains the germ of Napier's great discovery. We append a literal translation of the passage in question :

"It is also of some use to know this property. If a series of numbers be arranged in a geometrical progression from unity and any two of the terms of that progression be multiplied together, the product will also be a term in the same progression; and its place will be at the same distance from the larger of the two factors that the lesser factor is from unity; and its distance from unity will be the same, minus one, that the sum of the distances of the two factors from unity is distant from unity. For, let $A, B, C, D, E, F, G, H, I, K, L$ represent any geometrical progression from unity, of which A is the unity; let D be multiplied by H , and let X represent the product. Take L in the given progression, which is at the same distance from H that D is from unity. It is to be demonstrated that X is equal to L ."

This proposition Archimedes proceeds to prove, giving also the proof of the second proposition quoted in the above translation. Now this amounts to neither more nor less than demonstrating that, given a geometrical progression, the product of any two terms can be found without going through the actual process of multiplication. The following would be an equivalent method of stating the second of the above propositions :

Take any geometrical progression starting from unity and underneath each term write its "distance from unity," placing a 0 underneath unity. Thus :

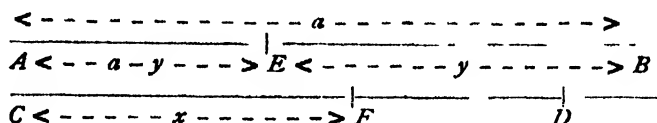
1,	2,	4,	8,	16,	32,	64,	128 . . .
0	1	2	3	4	5	6	7 . . .

Then, to multiply 4 by 16, we add 2 and 4 together and look up the number (64) above 6, which gives the required result. It is to be noted that by starting the lower progression at 0, we get rid of the "minus one" of the proposition as quoted by Archimedes.

But this—the study of the relation between an arithmetical and a geometrical progression—is precisely the manner in which the problem was approached by Napier. And his great insight is shown, both in the manner in which he obtained a progression or series of geometrical progressions such that the terms of the series were very near in value to the numbers in a table of natural sines—for it is to be remembered that primarily Napier was seeking for a table of logarithms of sines—and by

the ingenious manner in which he conceived his related arithmetical and geometrical series to be developed.

This latter relation is treated in a manner which strongly recalls Newton's subsequent development of the fluxionary calculus and may fitly be described here, leaving the question of the construction of the tables to be considered later. Translated into modern language and notation, Napier's treatment of the problem proceeds thus :



Imagine two lines AB and CD , AB of length equal to the radius, CD of indefinite length. Let two points start simultaneously from A and C with the same initial velocity. But whilst the velocity of C remains uniform, let that of A decrease in such a way that at any stage of the journey, such as E , its velocity is proportional to the distance EB yet to be described; when one point has reached F let the other be at E . Then CF is called the logarithm of EB .

The length EB is taken as the sine of a given arc and AB as the whole radius. It is clear, therefore, that the logarithm of radius—that is, the logarithm of the sine of 90° —is zero and that the logarithms increase as the sines decrease. The connexion between Napierian logarithms and logarithms to the base e —often wrongly called Napierian logarithms—may thus be exhibited in modern notation :

By definition,

$$CF = \log_N EB,$$

$$\text{i.e. } x = \log_N y.$$

Also, the velocity of $E = \frac{d(a-y)}{dt} = y$, by hypothesis, since Napier takes the constant of proportionality as unity.

Hence, integrating

$$-\log_e y = t + k.$$

To determine the integration constant we note that when $t = 0$, $y = a$ and therefore

$$k = -\log_e a.$$

Hence

$$t = \log_e \frac{a}{y}$$

Now the initial velocity of C = Initial velocity of $E = a$ and the velocity of C is uniform.

Therefore

$$\frac{dx}{dt} = a \text{ and } x = at,$$

the constant of integration vanishing, since x and t vanish together. Hence

$$t = \frac{x}{a} = \log_e \frac{a}{y},$$

or

$$x = \log_e y = a \log_e \frac{a}{y}$$

a is the radius, which Napier took as 10^7 units in length. So that we finally obtain

$$\log_N y = 10^7 \log_e \frac{10^7}{y}.$$

Now let us see how the actual tables constructed by Napier were evolved. In the two rows of figures previously cited the logarithms proceed in arithmetical progression, the numbers in geometrical progression and such a geometrical progression as we have cited shows increasingly large gaps. The problem is to construct a series of numbers in geometrical progression which shall yet be sufficiently close together to represent the natural numbers or rather, in Napier's case, to represent the sines of continually decreasing arcs, for, as has been said, Napier's final object was the construction of a canon of logarithms of sines. The manner in which this problem was solved can best be demonstrated by a brief analysis of the more important parts of Napier's posthumously published work, the *Constructio*, which we now proceed to give.

The full title of this work, which was, as has been noted, published posthumously in 1619, is, literally translated—"The Construction of the Wonderful Canon of Logarithms; and their relations to their own natural numbers; with an Appendix as to the making of another and better kind of Logarithms. To which are added Propositions for the solution of Spherical Triangles by an easier method: with Notes on them and on the above-mentioned Appendix by the learned Henry Briggs.

"By the Author and Inventor, John Napier, Baron of Merchistoun, etc., in Scotland."

Both this work and the *Descriptio* are, curiously enough, the most neglected of Napier's works. This neglect is, of

course, mainly to be ascribed to the early introduction of the more convenient tables computed to the base 10.

Three editions of the *Descriptio* were published in the years 1614, 1619 and 1620 respectively; an English translation by Edward Wright was published in 1616; a retranslation, together with a table of hyperbolic logarithms, was published in Edinburgh in 1857. A reprint of the Latin text is to be found in the sixth volume of Baron Francis Maseres' massive compilation entitled *Scriptores Logarithmici*. (Dates of publication 1791-1807.)

The *Constructio* is much less accessible. After the first (Edinburgh) edition of 1619 the only other edition of the Latin text was printed at Lyons in 1620. In 1889, however, a careful translation into English was issued by W. R. Macdonald, to which was added a very full and complete bibliography of Napier's published works.

The *Constructio* was printed in the form of a sequence of propositions, some sixty in number. Starting with a definition of progressions, both arithmetical and geometrical, Napier lays down, in very clear fashion, various rules for obtaining accuracy in computation, *e.g.* the taking of a large radius in order to get a larger number of significant figures in the numbers for both sines and logarithms; and, equally important, the annexing to the radius of a number of cyphers following a decimal point, the figures following the decimal point being discarded in the final tables. As Napier expresses himself in Proposition IV., "Thus, in commencing to compute, instead of 10,000,000 we put 10,000,000'0000000 lest the most minute error should become very large by frequent multiplication."¹

Then follows a clear discussion on the limits of accuracy obtainable in adding, subtracting, multiplying and dividing two numbers whose limits of accuracy are given and, beginning with Proposition XIII., the methods of forming "easy" geometrical progressions are carefully discussed.

Propositions XVI.—XXI. are concerned with the formation of three tables of fundamental importance. The First Table is a geometrical progression of 100 terms, of which radius forms the first term, consecutive terms being in the proportion

$$\frac{10000000}{9999999} \text{ or } \frac{\text{radius}}{\text{radius} - 1'}$$

¹ *Constructio*, Prop. IV.

This table is readily formed by subtracting "from radius with seven cyphers added . . . its 10,000,000th part and from the number thence arising its 10,000,000th part and so on."¹ A specimen of part of the First Table, showing its construction, is given in the accompanying figure.

First Table

(1)	10000000 0000000
	<u>1 0000000</u>
(2)	9999999 0000000
	<u>9999999</u>
(3)	9999998 0000001
	<u>9999998</u>
(4)	9999997 0000003
	<u>9999997</u>
(5)	9999996 0000006
	<u>9999996</u>
	and so on, up to the 100th term, which is
(100)	9999900 0004950

The Second Table is a geometrical progression of fifty terms, radius (with *six* cyphers added) forming the first term, the numbers being in the continued proportion of the first term to the last term in the First Table; that is, in the proportion of 100,000 to 99,999. This table again may be formed "with sufficient exactness by adding six cyphers to radius and continually subtracting from radius its 100,000th part in the manner shown."²

Second Table

(1)	10000000 0000000
	<u>100 000000</u>
(2)	9999900 0000000
	<u>99 999000</u>
(3)	9999800 0010000
	<u>99 998000</u>
(4)	9999700 0030000
	<u>99 997000</u>
(5)	9999600 0060000
	<u>99 996000</u>
	and so on, up to the 50th term, which is
(50)	9995001 222927 ³

The Third Table is much more extensive, consisting of sixty-nine columns, each column containing twenty-one terms.

¹ *Constructio*, Prop. XVI.

² *Ibid.*, Prop. XVII.

³ This number is erroneous (*Vide* Note B, Appendix).

Considering any one column, the terms comprising it are in the continued proportion of the first term to the last term in the Second Table ; that is, in the proportion of 10,000 to 9,995. The first term of the first column is radius "with four cyphers added." The succeeding terms of the first column being in the above ratio, are easily computed by methods strictly analogous to those discussed in the formation of the First and Second tables and the twenty-first term is found to be 9,900,473'5780.

The *first* numbers in *each* of the 69 columns are approximately in the proportion of the first term to the twenty-first term of the first column, *i.e.* in the proportion of 100 to 99. The numbers which head each of the columns are therefore readily calculated and the remaining twenty terms in each column are then easily filled in, as they form a descending geometrical progression, of which the first term is given, consecutive terms being, as stated above, in the ratio of 10,000 to 9,995. Thus the table, when completed, has the form shown below :

Third Table

Terms.	Column I.	Column II.	Column III	etc. . . . till.	Column LXIX.
1	10000000'0000	9900000'0000	9801000'0000	etc.	5048858'8900
2	9995000 0000	9895050'0000	9796099'5000		5046334'4605
3	9990002'5000	9890102'4750	9791201'4503	etc.	5043811'2932
4	9985007'4987	9885157'4237	9786305'8495		5041289'3879
5	9980014'9950	9880214'8451	9781412'6967	etc.	5038768'7435
etc.	etc. up to the	etc.	etc.	etc.	etc.
up to	21st term which	etc.	etc.		etc.
	is	till	till		till
21	9900473'5780	9801468'8423	9703454'1539	etc.	4998609'4304

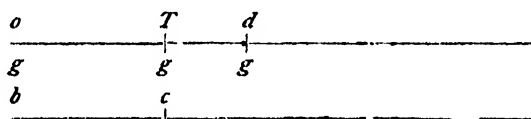
The net result of all these computations is that in the Third Table we have, between radius and (approximately) half-radius, interposed 68 numbers in the continued proportion of 100 to 99; and between *each* pair of these numbers we have interposed twenty other terms in the continued proportion of 10,000 to 9,995. Also, between the first two numbers of the Third Table, which are also the first and last of the Second Table, we have interposed 48 numbers in the continued proportion of 100,000 to 99,999. And, finally, between the first two numbers of the Second Table, which are also the first and the last of the First Table, are interposed 98 numbers

in the continued proportion of 10,000,000 to 9,999,999. Thus we have in these three tables a series of numbers in geometrical progression, which numbers also coincide very nearly with those in a table of natural sines from 90° to 30° .

It remains to show how, to each of these "natural" numbers, Napier appended the corresponding "artificial" number or logarithm.¹ The portion of the *Constructio* (§§ XXII.-XXVI.) immediately following the discussion of the formation of the three tables given above is concerned with the definition of logarithms which we have previously explained. Proposition XXVII. proves, as before mentioned, that nothing is the logarithm of radius (*i.e.* of the sine of 90°). Proposition XXVIII. is of fundamental importance; as an illustration of Napier's methods, we proceed to give his proof, as far as possible in his own manner.

The proposition states that, if r be radius and s any given sine, then the logarithm of s is greater than $r - s$ and less than

$$(r - s) \frac{r}{s}.$$



Let TS represent radius, and let a point g start from T with a velocity proportional to TS , its velocity when at any point d being proportional to dS , dS being taken to represent any given sine s . Simultaneously with the departure of g from T , another point a moves from b with a uniform velocity equal to the initial velocity of g ; if, then, when g is at d , a is at c , bc is called the logarithm of dS .

In his proof of the proposition quoted above, Napier produces the line ST to o , so that oS is to TS as TS is to dS .

Hence it follows that oT is equal to $(r - s) \frac{r}{s}$; and since Td is equal to $r - s$, we have to prove that bc is greater than Td and less than oT . This Napier proves by assuming that the moving point g starts from o , its velocity decreasing according to the geometrical law in such a manner that when g arrives

¹ See Appendix C.

at T it has the velocity (proportional to TS), with which a starts from b . In his own words:¹

"For in the same time that g is borne from o to T , g is borne from T to d , because oT is such a part of oS as Td is of TS and in the same time (by the definition of a logarithm) is a borne from b to c ; so that oT , Td and bc are distances traversed in equal times; but since g when moving between T and o is swifter than at T , and between T and d slower but at T equally swift with a ; it follows that oT the distance traversed by g moving swiftly is greater and Td the distance traversed by g moving slowly is less than bc the distance traversed by the point a with its medium motion, in just the same moments of time; the latter is, consequently, a certain mean between the two former."

The proposition can, of course, be demonstrated by means of the relation already proved that

$$\log_{Ns} = r \log_e \frac{r}{s};$$

from which we can easily show that

$$(r-s) \frac{r}{s} > r \log_e \frac{r}{s} > r-s,$$

by writing the intermediate term in the form $r \log_e \left(1 + \frac{r-s}{s}\right)$ and expanding. When r is very nearly equal to s , the logarithm of s is, therefore, very nearly equal to the arithmetic mean of the limits, i.e. is very nearly equal to $\frac{(r+s)(r-s)}{2s}$.

This proposition Napier uses at once to find the logarithm of the second term in the First Table, for, the first term being radius, its logarithm is, by definition, zero; and the logarithm of the second term lying between the above limits lies therefore between $r-s = 1'0000000$ and $(r-s) \frac{r}{s} = \frac{10000000}{9999999} = 1'0000001$ and may therefore be taken with sufficient exactness as $1'00000005$. And this is also the common difference for the logarithms of every number in the First Table; hence, multiplying this common difference by 2, 3, etc., the logarithms are readily appended to the 100 terms constituting the First Table.²

To proceed from the First to the Second Table, another

¹ *Constructio*, Prop. XXVIII.

² *Ibid.* Prop. XXIX.-XXXIII.

proposition is employed, that, given two sines s_1 and s_2 , the *difference* between the logarithms of the two sines lies between the limits $(s_1 - s_2) \frac{r}{s_2}$ and $(s_1 - s_2) \frac{r}{s_1}$, which difference, if the two sines differ but slightly, may be taken with sufficient accuracy to be equal to the arithmetic mean of the above limits.¹ This proposition, which Napier proves in much the same manner as the proposition quoted previously, may be verified readily by putting

$$D = \log_N s_2 - \log_N s_1 = r \log_e \frac{s_2}{s_1} = r \log_e \frac{1 + \frac{s_2 - s_1}{s_1}}{1} = r \log_e \left(1 + \frac{s_2 - s_1}{s_1} \right)$$

and putting D in the form

$$D = r \log_e \left(1 + \frac{s_2 - s_1}{s_1} \right) = r \log_e \left(1 + \frac{s_2 - s_1}{s_1} \right)$$

and expanding, the truth of the proposition is easily shown.

Now the last term of the First Table being 9,999,900'0004950 and the second term of the Second Table 9,999,900, when these numbers are substituted in the expression for the arithmetic mean of the limits given above, it is found that the difference of the logarithms of these two terms is, to the approximation considered, '0004950; and, adding this number to the logarithm of the last term of the First Table gives 100'0005000 at the logarithm of the second term of the Second Table. Since the logarithm of the first term of the Second Table is zero, this number gives us also the common difference for all the terms in the Second Table.

By a precisely similar process we can pass from the Second Table to the first column of the Third Table and fill in the logarithms of the twenty-one terms of this column. Then, using the theorem again to pass from the twenty-first term of the first column to the first term of the second column, we find that the logarithm of this latter term is 100503'3; this number, it must be noticed, is the common difference of the logarithms of the first terms of the first, second . . . sixty-ninth columns, of the second terms of the various columns, and so on; so that, knowing the logarithms of all the terms of the first column and the common difference between all terms on the same line in the various columns, we can fill in the logarithms of all the terms of the Third Table. The Third Table, with its logarithms so

¹ *Constructio*, Prop. XXXIX.-XL.

appended, Napier calls the "Radical Table"; a specimen of the part of this table given in the *Constructio* is shown below :¹

The Radical Table

Column I.		Column II.		Column I.XIX.	
Natural numbers.	Logarithms.	Natural numbers.	Logarithms.	Natural numbers.	Logarithms.
10000000'0000	'0	99000000'0000	100503'3	5048858'8900	6834225'8
9995000'0000	5001 2	9895050'0000	105504'6	etc.	6839227'1
999002'5000	10002'5	9890102'4750	110505'8	5043811'2932	6844228'3
9985007'4987	15003 7	9885157'4237	115507'1	etc.	6849229'6
9980014'9950	20005'0	9880214'8451	120508'3	5038768'7435	6854230'8
etc. up to	etc. up to	etc. up to	etc. up to	etc. up to	etc. up to
9900473'5780	100025'0	9801468'8423	200528'2	4998609'4034	6934250'8

With the help of this Radical Table it is an easy matter to obtain the logarithm of any given sine, as, given the sine, the number nearest to it in the Radical Table must be noted and by the "difference theorem" quoted above the difference between the logarithms of the two numbers may be found. Adding this difference to or subtracting it from the logarithm of the number in the Radical Table at once gives the logarithm of the given sine.

From this Radical Table, therefore, the logarithms of the sines of all angles between 90° and 30° are computed. Further than this we cannot go, without other assistance, as the natural numbers in the Radical Table only go down to (about) half-radius, which is the sine of 30° . It remains, then, to explain the methods adopted by Napier in computing the logarithms of the sines of the angles between 30° and 0° .

Two methods are indicated by means of which the computation may be effected. In the first,² the given sine x , which, by hypothesis, is the sine of some angle less than 30° , is multiplied by some definite number b , the number b being so chosen that the product $bx (=y, \text{ say})$ lies within the limits of the Radical Table. This being so, the number nearest to y is looked up in the Radical Table and by the "difference theorem" earlier quoted the logarithm of y may be evaluated. Then, knowing the logarithms of y and of b , the value of the logarithm of x is obtained from the equation $y = bx$.

In the second method,³ Napier utilises the proposition that "As half-radius is to the sine of half a given arc, so is the sine

¹ *Constructio*, Prop. XLVII.

² *Ibid.* Prop. LI.-LIV.

³ *Ibid.* Prop. LV.

of the complement of the half-arc to the sine of the whole arc." This is, of course, the Napierian equivalent of the trigonometrical theorem

$$\sin 2\theta = 2 \sin \theta \cos \theta,$$

which equation, written in the form

$$\sin \theta = \frac{\sin 2\theta}{2 \sin (90 - \theta)}$$

enables us at once to compute the logarithm of $\sin \theta$, knowing the logarithms of the sines of 2θ and of $(90 - \theta)$; choosing θ so that 2θ , to begin with, lies within the limits of the Radical Table, the table may be gradually extended so as to include the logarithms of the sines of all angles from 30° to 0° . It is further pointed out¹ that this method can be used for all angles less than 45° ; so that the construction of the logarithms of the sines of the angles between 45° and 30° is thereby rendered much more simple, the use of the "difference theorem" and the Radical Table being avoided.

It is hoped that the preceding analysis will suffice to show the uniqueness and originality of Napier's great discovery. The publication of the *Descriptio* in 1614 was hailed with an amount of enthusiasm and the full credit of Napier's work awarded to him with a unanimity seldom paralleled in the annals of mathematical discovery. After the fashion of the times, the enthusiasm of Napier's contemporaries found vent in a number of laudatory poems, of which one by Thomas Bretnor possesses sufficient merit, apart from its somewhat too-fervid patriotic spirit, to bear reproduction to the extent of a couple of verses:

"And bonnets vaile, you Germans! Rheticus,
Reignoldus, Oswald, and John Regiomont,
Lansbergius, Finckius and Copernicus,
And thou, Pitiscus, from whose clearer font
We suckéd have the sweet from Hellespont.
For were your labours ne'er composed so well
Great Napier's worth they could not parallel.
By thee great Lord we solve a tedious toyle,
In resolution of our trinall lines,
We need not now to carke, to care, or moile,
Sith from thy witty braine such splendor shines,
As dazels much the eyes of deepe divines.
Great the invention, greater is the praise,
Which thou unto thy nation hence doth raise "

¹ *Constructio*, Prop. LVIII.

But the tables of Napierian logarithms had hardly seen the light before proposals were put forward for altering the base to the more convenient number 10 also making zero the logarithm of unity and unity the logarithm of 10, so that numbers and their logarithms should increase and decrease together. In giving an account of this change, Hutton, always learned and usually extremely accurate, does less than justice to Napier, as he assumes that Napier's part in recommending this important alteration was practically nil and that jealousy of Napier's work existed on the part of Briggs, which certainly seems to have no foundation in fact. Mark Napier, in his *Memoirs of John Napier*, successfully refutes Hutton's conclusions but "falls into the opposite error of reducing Briggs to the level of a mere computer."

Without going exhaustively into the evidence, it would seem sufficient to say that, taking the words of both Napier and Briggs at their face value, the change by which 0 became the logarithm of radius and the logarithm of the tenth part of radius became 10,000,000,000 was the separate and independent idea of each writer; and that Napier further suggested that 0 should become the logarithm of unity and 10,000,000,000 that of the whole sine.

Brigg's account of the matter is given in the preface to his *Arithmetica Logarithmica* (1624):

"... I myself, when expounding publicly in London their doctrine to my auditors in Gresham College, remarked that it would be much more convenient that 0 should stand for the logarithm of the whole sine, as in the canon Mirificus, but that the logarithm of the tenth part of the whole sine, that is to say, 5 degrees 24 minutes and 21 seconds, should be 10,000,000,000. Concerning that matter I wrote immediately to the author himself; and, as soon as the season of the year and my vacation time of my public duties of instruction permitted, I took journey to Edinburgh, where, being most hospitably received by him, I lingered for a whole month. But as we held discourse concerning this change in the system of logarithms, he said that for a long time he had been sensible of the same thing and had been anxious to accomplish it,¹ but that he had published those he had already prepared, until he could construct tables more convenient, if other weighty matters and his frail health would permit him to do so. But he conceived that the change ought

¹ "Cum autem inter nos de horum mutatione sermo haberetur, ille se idem dudum sensisse et cupivisse dicebat."

to be effected in this manner, that 0 should become the logarithm of unity, and 10,000,000,000 that of the whole sine; which I could not but admit was by far the most convenient of all. So, rejecting those which I had already prepared, I commenced, under his encouraging counsel, to ponder seriously about the calculation of these tables."

The notably high characters of both Napier and Briggs, the strong friendship which existed between the two writers and the unqualified admiration and veneration which Briggs ever shows of his master, justify us in taking these words at their plain meaning; it may be added that a more exhaustive study of the evidence afforded serves to confirm the views stated above.

It is not necessary here to go into any great detail concerning the manner of computation of logarithms to the base 10, as a clear account of some of the methods used is easily accessible in the article "Logarithms" in the *Encyclopædia Britannica*. Thus, for example, in computing the logarithm of 5, given $\log 1$ and $\log 10$, the geometric mean of 1 (A) and 10 (B) is taken, giving $C = \sqrt{AB}$. Then, finding $D = \sqrt{BC}$, $E = \sqrt{CD}$, etc., we finally arrive at a mean which may be made to approach as closely as we please to the value 5'00000. . . And to every geometric mean there corresponds a logarithm obtained by continually taking arithmetic means of the logarithms in like manner, finally giving

$$\log 5'000000 = '6989700.$$

A second method is outlined by Napier in the Appendix to the *Constructio*. In his own words—

" . . . the Logarithm of any given number is the number of places or figures which are contained in the result obtained by raising the given number to the 10,000,000,000th power.

"Also if the index of the power be the Logarithm of 10 the number of places, less one, in the power or multiple, will be the Logarithm of the root.

"Suppose it is asked what number is the Logarithm of 2. I reply, the number of places in the result obtained by multiplying together 10,000,000,000 of the number 2.

"But, you will say, the number obtained by multiplying together 10,000,000,000 of the number 2 is innumerable. I reply, still the number of places in it, which I seek, is numerable.

"Therefore, with 2 as the given root, and 10,000,000,000 as the index, seek for the number of places in the multiple, and not for the multiple itself; and by our rule you will find 301,029,995, etc., to be the number of places sought, and the Logarithm of the number 2."

A method used by Briggs for finding the logarithms of small prime numbers, which depended upon the formation of a large number of geometric means between unity and the given prime number, is fully outlined in the article "Logarithms" above-mentioned and needs no further discussion here.

By these methods Briggs computed the logarithms of all integers from 1 to 20,000 and from 90,000 to 100,000 to 14 places of decimals. The gap from 20,000 to 90,000 was filled by the calculations of Adrian Vlacq, who computed his logarithms to 10 places of decimals.

It is interesting to note that an abusive mention of Vlacq by Milton in his *Defensio secundo pro populo Anglicano* led Vlacq to state simply and clearly the story of his life from the age of 26. Any faithful account of one to whom mathematicians are so much indebted—for the tables of Briggs and Vlacq are the parents of all the logarithmic tables which have succeeded them, no re-computation on such an extensive scale having been made since—must necessarily possess great interest, and "one is almost inclined to pardon Milton his abuse, seeing that thereby we are made acquainted with what would otherwise probably have always remained a mystery."¹

Here an account of the genesis of logarithms may fitly close. Several points of minor interest remain—a consideration, for example, of Kepler's logarithmic tables, which differ from Napier's in one point only; in Napier's Table the *arc* of the quadrant is divided into a definite equal number of parts, so that the sines corresponding to these angles are, in general, irrational numbers. In Kepler's table the *radius* is divided into a definite number of equal parts, so that the sines are rational numbers, the corresponding angles or arcs being irrational.

Something might be said also of the very doubtful claim of Joost Burgi (1552-1632) to be an independent discoverer of logarithms, a discussion of which may be found in several of the standard histories of mathematics.

¹ Glaisher, *Phil. Mag.* October 1872.

But, before concluding, one widespread error calls for notice. In many text-books on trigonometry we find the statement that, to avoid the inconvenience of printing negative characteristics, the number 10 is always added to the logarithms of sines, cosines, etc., thus giving the so-called logarithmic sines, etc. This is by no means an exact statement of the facts. The trigonometrical tables most in use at the beginning of the seventeenth century were constructed, as previously explained, to the radius 10^{10} . It follows, therefore, that the logarithm of radius (the sine of 90°) is 10, with corresponding numbers having as characteristics 9, 8, . . . etc., for the remaining sines. The numbers, therefore, given as "tabular logarithms" in a modern book of tables are *actual* logarithms, the manner of printing them having never been altered; the modern conception of the trigonometrical functions as ratios gives us, however, unity as the sine of 90° ; consequently, the tabular logarithms as printed are, in every case, too great by ten. It seems a pity that the account of such an interesting remnant of seventeenth-century usage should be obscured by the usual "explanation" of trigonometrical text-books.

APPENDIX

NOTE A

The following short list of authorities consulted may be useful to those who wish to pursue the subject further.

NAPIER, *Mirifici Canonis Logarithmorum Descriptio*. First edition, 1614. A reprint is contained in vol. vi. of Baron Francis Maseres' compilation entitled *Scriptores Logarithmici*. English translations made by Edward Wright (1616) and Herschell Filipowski (1857).

NAPIER, *Mirifici Canonis Logarithmorum Constructio*. First Edition, 1619. English translation, together with an exhaustive bibliography of Napier's works, made by W. R. Macdonald (1889).

HUTTON'S *Mathematical Tables*. A full and for the most part accurate historical introduction is prefixed to the earlier editions of the above Tables. This introduction is reprinted in Hutton's *Mathematical Tracts*, vol. i. (1812).

NAPIER, MARK, *Memoirs of John Napier of Merchiston* (1834).

Encyclopædia Britannica. Tenth edition. Articles Logarithms, Mathematical Tables, Napier, etc.

MONTUCLA, *Histoire des Mathématiques*; completed and published by Lalande (1799-1802).

FINK, *Geschichte der Elementar-Mathematik*. English translation by Beman and Smith (1903).

CAJORI, *A History of Mathematics*.

BALL, A Short History of Mathematics.

DE MORGAN, Trigonometry and Double Algebra.

GLAISHER, Articles in the Philosophical Magazine for Oct. and Dec. 1872, and May 1873.

NOTE B

The fiftieth term in Napier's Second Table, given as 9,995,001'222927 is incorrect, the true value being 9,995,001'224804.

This, of course, introduces a corresponding error into the logarithms attached to the Radical Table, inasmuch as we have seen that the logarithm of the first proportional in the Radical Table is obtained from the logarithm of the last proportional in the Second Table by means of a theorem which involves the difference of the proportionals; and, one of the proportionals being in error, the logarithm will also be incorrect. The magnitude of the error introduced may be shown by noting that the logarithm of the last term in the Radical Table is given as 6,934,250 8, its true value being 6,934,253'4—an error of rather less than one in $2\frac{1}{2}$ millions.

The mistake, unnoticed by Hutton, seems to have been first pointed out by Biot in 1835 and later in 1865 by Sang.

NOTE C

The derivation of the word logarithm is not without interest. Even when we know that logarithm = λόγων ἀριθμός = "the number of the ratios," the modern mode of deriving logarithms as powers, and of computing logarithms by means of series, is apt to render the meaning underlying the phrase "number of the ratios" somewhat obscure. But the originators of the word looked at the subject of logarithms from the point of view of compounded ratios. Suppose, then, that the ratio of 10 to 1 is compounded of, say, a million small ratios or ratiunculae, each of which is, of course, the millionth root of ten. Then the ratio of 2 to 1 is compounded of 301,030 of these small ratios, so that the logarithm of 2 is given by the number of the ratios or ratiunculae which is contained in the ratio of 2 to 1. Hence the word logarithm.

It may be noted that whilst Napier uses the word Logarithmus in the *Descriptio*, published in 1614, he uses, in the text of the posthumously published *Constructio*, the phrase Numerus artificialis, or simply Artificialis, as opposed to Numerus naturalis for the ordinary numbers. The term Logarithmus, however, is used in the title-page, headings and Appendix to the *Constructio*.

REVIEWS

The Disorders of Post-Natal Growth and Development. By HASTINGS GILFORD, F.R.C.S. [Pp. xxii + 727.] (London: Adlard & Son, 1911. Price 15s. net.)

MR. GILFORD in this work propounds the novel thesis that all post-natal disease is primarily inherent, though it may be aggravated by outside agencies: disease, he contends, is the expression of an exaggeration of phases in the normal life-history of cells.

A curious and not uninteresting faculty of looking on the wrong side of things and a tendency to place effect before cause are the keys to the "theories" with which the book abounds. Thus in dealing with the influence of heredity on post-natal disorders, the statement is made that the characters latest acquired are those most easily lost; such elementary logic, of which examples abound throughout the book, is contrary to the whole experience of practical stock-raisers and of professed students of the principles of heredity.

The statement is often made that the constituent cells of essential organs are capable of degenerating individually yet of continuing to live in altered and according to the author more primitive forms: thus we read—"Cancer, cirrhotic liver, acromegaly, though seemingly possessing nothing in common, are all examples of the same morbid variation—*i.e.* a premature old age of groups of cells." It is scarcely necessary to point out that so little is known as to the nature of cancer that no such extreme statement can be justified; cirrhosis of the liver is well known to be the result of the replacement of dead liver-cells by fibrous tissue after their degenerate remains have been removed by phagocytes; and it is accepted that acromegaly is due to interference with the internal secretion of a part of the pituitary body. The exactness of the resemblance between these conditions would seem hard to seek.

Again, we are told that fullness of blood and excess of red blood corpuscles result in a tendency to apoplexy. It is not generally recognised that the rare disease Polycythæmia Rubra quoted by the author in support of this statement is alone responsible for death from cerebral hæmorrhage or thrombosis.

As a final example of the author's peculiar views, aberrant growth may be quoted. Abnormal growth is most excessive at the period of greatest relative activity. Hence the astounding application: "We must look for hypertrophy of the pylorus shortly after birth when the stomach, a new and untried organ, comes first into use," springing at a bound, as the author says, into activity. Criticism of this flight of the imagination seems needless.

No doubt much labour has been expended by the author in compiling this remarkable book; it is to be regretted, however, that neglect to consider internal secretions of organs and the profound effect of bacterial infection render the volume almost worthless. The book, one of great length, is clearly printed; the illustrations—few of which are original—are well reproduced; and it is provided with an exhaustive index.

R.

Animal Life: Reptiles, Amphibia, Fishes and Lower Chordata. Edited by J. T. CUNNINGHAM, M.A., F.Z.S. [Pp. xvi + 510, with four plates in colour and numerous other illustrations.] (London: Methuen & Co. Price 10s. 6d. net.)

THIS volume, like others of the series, is written from an evolutionary point of view, the section on Reptiles by Mr. Lydekker, F.R.S.; that on Amphibia and Fishes by Mr. Boulenger, F.R.S., and Mr. Cunningham; and the remaining sections on the Lampreys, Hag-Fishes, Sea-Squirts and other primitive or degenerate relatives of the vertebrates by Prof. J. Arthur Thomson—all well-known specialists of eminence. It is well printed, admirably illustrated and not so overladen with china-clay that it cannot be held, though somewhat too heavy to handle comfortably. The book is full of fascinating information and should not only command a wide circle of adult readers but also be of real service in schools. No better prize or gift-book could be given to an intelligent boy, especially to one who has a taste for natural history. As the general editor of the series remarks in his brief preface, "some of our neighbours assure us that 'Darwinism' is dead! If these pages show anything they show that the contrary is emphatically the case!"

The Life of the Plant. By C. A. TIMIRIAZEFF. Translated from the revised and corrected seventh Russian edition by Miss A. Chéréméheff. [Pp. xvi + 355.] (London: Longmans, Green & Co. Price 7s. 6d. net.)

IN this book, Prof. Timiriazeff has reproduced a course of lectures he delivered in Moscow in 1876 with the object, he says, "of informing the public," in a popular way, of the then state of vegetable physiology. He appears to have been fully conscious of the difficulty of the task he had undertaken—that it was necessary that the author of such a review, as he expresses it, should "give up for a while his usual point of view, that of a specialist; and should, so to speak, step back a little in order to see what science looks like at a distance." The book is worth its cost for this precious sentence alone. Our writers of text-books, as a rule, have no sense of perspective: if they would only step back at times and contemplate their work from a distance, they might see how forbidding its appearance is to the intelligent reader. One reason why science, at the present day, is making little or no headway—why those who are set in authority over us are so lamentably ignorant of its methods and of its teachings—is that its devotees, with few exceptions, are so steeped in their professional jargon that they are incapable of expressing themselves in clear and simple terms that the multitude can understand. We trust that the praiseworthy example set by a Russian writer will not be without influence; at least it will show that it is possible to deal with difficult problems in a simple and attractive way.

The book is remarkable on account of the clearness and simplicity of its style and also of the admirable series of apt experimental illustrations that are given in explanation of the various processes considered. It is divided into ten chapters, in which are discussed the external and internal structure of the plant; the cell; the seed; the root; the leaf; the stem; growth; the flower; the plant and the animal; and the origin of organic forms. In a final chapter, the plant is considered as a source of energy. The work of translation has been most admirably done.

In the English preface, Prof. Timiriazeff rightly protests against the alarming spread of the "Reizphysiologie," with its morbid outgrowth of "Neovitalism"

and "Phyto-psychology" and their natural corollary, anti-Darwinism. "I am as firmly convinced," he says, "as I was forty years ago, that the 'mechanistic conception' and Darwinism have been bequeathed by the 'wonderful century' to the still infant science of plant physiology as the two sure guides for its further evolution."

Here and there are passages to which objection can be taken as a little out of date perhaps, if not incorrect; such, however, are rare. The reference, at p. 167, under osmotic pressure, to albumen and gum as being productive of the same effect as sugar is a case in point. Clarity of argument and of statement, however, are main characteristics of the work.

In these days, when so many are interested in the practice of horticulture, such a book should meet with a most cordial reception from all who desire to gain some understanding of the life history of plants; it should also be of great service in schools.

One of its chief advantages is that Prof. Timiriazeff has known what to leave out. He has not attempted to make details clear but has dealt broadly with the various problems.

Monographs on Biochemistry. The Chemical Constitution of the Proteins.

Part I. Analysis. By R. H. A. PLIMMER, D.Sc. Second edition. [Pp. xii + 188.] Price 5s. 6d. net.—**The Physiology of Protein Metabolism.** By E. P. CATHCART, M.D., D.Sc. [Pp. viii + 142.] (Price 4s. 6d. net.) London: Longmans, Green & Co.

DR. PLIMMER has increased the value of his now well-known monograph by giving a more detailed description of the methods followed in analysing the proteins and has brought his account up to date in other respects. Reference is made in the preface to the astonishing activity displayed by Abderhalden. Dr. Cathcart gives a list of no fewer than fifty communications published under this worker's name up to the close of the year 1910. The two books under notice serve to bring out very clearly the almost superficial character of much of the work that has been done with proteins and the faults inherent in the German method, which unfortunately involves placing tasks of the utmost difficulty, time after time, in the untried and inexperienced hands of student operators. If we are to progress, the work must be done in a more thorough manner in future, more in accordance with the example set by the pioneer investigator in this field, Emil Fischer and his distinguished American follower Osborne.

Dr. Cathcart's is probably the most valuable monograph published in the series and is exceptionally well written. The subjects dealt with are the digestion and absorption of proteins; protein regeneration; feeding experiments with products of digestion free from biuret; the removal of the amino-group; influence of food on the composition of the tissues; protein requirements; theories of protein metabolism; starvation; and work. The work done in each of these chapters is noted and considered—somewhat hastily, it must be confessed, but none the less skilfully. The book has a fault which probably is inseparable from its size, too little being said of the manner in which the investigations considered were conducted to enable the reader to appraise their value as evidence. The vagueness of the conclusions arrived at in most cases is very apparent. On this latter account, the book will not appeal very strongly to the beginner; but it will be invaluable to the serious student in guiding him through the literature—much of

which, perhaps, might now be burnt with advantage. It is to be regretted that Dr. Cathcart has not given a crisp survey of the situation in a brief final chapter—he perhaps errs on the side of modesty throughout the volume.

Spices. By HENRY N. RIDLEY, M.A., C.M.G., F.R.S. [Pp. 449.] (Macmillan & Co. Price 8s. 6d. net.)

FEW among the vegetable products used in everyday life have a more romantic history than the spices, as they have played an important part from the very earliest times, first in instigating exploration and then in causing the founding of settlements. Most of the known spices are derived from the East—the Asiatic tropics.

It is characteristic of the times that the public know little about spices - their botanical origin, the methods of cultivating them and, perhaps happily, the frequency with which they are adulterated. The first two of these themes are very admirably treated in the work under notice and it may be recommended as pleasant reading to those who are prepared to skip judiciously whenever the writer lapses somewhat too freely into details regarding the methods of cultivation. Even these sections are interesting, as showing the difficulties to be encountered and, speaking broadly, the unscientific manner in which the cultivation of spices is still carried on. The author is director of the Botanic Gardens of the Straits Settlements and is therefore entitled to speak with authority on the subject he deals with. Throughout the work, the commercial aspect is not overlooked and careful statements are given of the cost of planting, upkeep and production of the crop and of the probable return. The book is written primarily for use by planters in all parts of the world and should prove very useful to them, as well as of interest to the many who by force of circumstances have become interested in the plantation industry of the Asiatic tropics. The spices considered are vanilla, nutmegs, cloves, pimento, cinnamon, cassia bark, pepper, cardamoms, capsicum, coriander, ginger and turmeric. The book might with advantage have been more fully illustrated but it is attractively printed.



Avlgent School

THE CONDITIONS OF RUSSIAN AGRICULTURE

BY J. VARGAS EYRE, PH.D.

EXCEPTING a few commercial travellers, not many Englishmen go so far afield as to visit Russia in their wanderings. In a measure this is because a belief prevails that travelling is rendered almost intolerable by the overbearing attitude of the police and other officials. Moreover little information is available, in the ordinary way, which bears the stamp of personal knowledge and the prospect of having to find his own way and shift for himself is not an inviting one to the tourist. In short, want of knowledge of the country has led most people to regard Russia with suspicion, if not as forbidden ground. The accounts presented in the daily papers do not in any way tend to mitigate the feelings of mistrust of the people which undoubtedly exist; in fact, our knowledge of Russia and the Russians is superficial and often false and as we visit them so rarely and have so little authentic information of their doings, this is not surprising.

Those who wish to learn what Russia is should go there with an open mind; they should visit the peasant, the village and the small town but not the cities; above all they should avoid St. Petersburg, which is the headquarters of officialdom—a city of “Tchin,” beautiful but not Russian. The true Russia is to be found away in the vast and silent plains, where dwell the peasants, who form seventy-five per cent. of the entire population—one hundred and twenty million souls, mostly engaged in husbandry, thinly scattered over a vast Empire.

To understand the position of Russian agriculture, it is necessary to acquire an understanding of the peasant and to remember that servitude was abolished but fifty years ago. By the emancipation of the serfs more than twenty-two million people were delivered from bondage and a new era was opened up. Millions of bondservants became peasant agriculturists on the Communal System and thousands rented land for themselves. Being a deeply religious people but steeped in superstition,

having few requirements and knowing nothing of luxury, they naturally made agriculture subservient to the enjoyment of their freedom. Withheld from all knowledge of progress and purposely kept ignorant, they were scarcely able to bear the burden of their own existence, let alone fight for betterment. Consequently, it cannot be said that the hopes of the pioneers of 1861 have been realised. The onus of failure must rest with the clergy and the bureaucracy; had it not been for the ignorance and arrogance of a host of subordinate officials, the peasantry would long since have been in a better condition; as it is, they remain a sad monument of the past—crushed and kept crushed.

As a class they are careless and lazy, accepting defeat by any difficulty with a sigh of relief. Circumstances of government and conditions of climate have moulded them a listless people, whose annual office it is merely to scratch over the ground, sow seed and invoke the aid of the Almighty to afford them sufficient supplies to tide them over from harvest to harvest.

Such are the majority of Russian agriculturists but a minority are lifting themselves and among these the pessimism and apathy that have so long prevailed are giving place to a spirit of hopeful enterprise. Signs are not wanting, in fact, that Eastern languor is departing before the encroaching influence of Western ideas. In some districts, more especially in the south and south-eastern provinces, agriculture has been raised to quite a high level, the people being no longer satisfied to supply only the bare necessities of their own household or the requirements of the village community; but on the whole, the standard of agriculture is still very low, only about ten or twelve per cent. of peasant farmers being able to afford to sell part of their produce.

The Russian Empire is so vast in extent and includes so many varieties of soil and extremes of climate that to generalise further would be to create a false impression. It is, however, necessary to realise how great are the undeveloped agricultural resources of the country and these forewords may assist readers to view things Russian in their proper perspective.

In the north of Russia, forest extends for hundreds of miles with scarcely any interruption and it is said that the greater part of the region has not been explored by civilised man. Winter continues through nearly eight months of the year, so that it is doubtful whether any attempt will be made to carry on farming operations against such heavy odds. To the south of

this region, agriculture is practised but it is only of the most primitive order. The soil is poor and the peasants have nothing wherewith to enrich it. During nearly seven months out of the twelve, the land is held in the grip of winter and much of the open period is affected by cold rain. The land at present cultivated in the district is mostly farmed in small plots, which are rented by the peasants, who work them as it suits their convenience. The plots are dotted about in the scrub, advantage being taken of any natural shelter this offers and of favourable variations in the soil. Owing to migration of the peasantry to more congenial conditions, the northern parts of the country are very sparsely populated; standing there beside one cultivated plot, it is seldom possible to see another. When serfdom prevailed, a far greater proportion of the land was under cultivation, so that probably only the best is still worked. Ploughing is often done entirely by human labour, the plough, a simple implement of wood, being pulled and pushed across the small field by the capable members of the household. Seldom is the land given any dressing of manure, because cattle are scarce. Year after year the same plot of land is scratched over and a crop raised; the miserable crop is sometimes a little better, sometimes a little worse than usual but the peasant says nothing and accepts as inevitable the small success which attends his labour. Generally oats or barley are grown but the crops are very poor indeed both as regards yield of grain and straw. It is a common thing to see fully grown crops of oats standing no higher than ten or twelve inches and carrying but little grain. Artificial manure is seldom used because so few can afford the outlay; the farmers possessed of small capital who farm the very light soil between Vologda and Moscow apply a dressing of some 3 cwt. of kainite to the acre and reap a benefit of a 30 per cent. increased yield.

The best results are obtained with flax; though not so adverse to the production of good fibre crops, the climate is not suited to the successful harvesting of seed. Much flax is grown in the district of which Vologda is the centre, more especially in the vicinity of the river Suhona, where, despite the poorness of the soil, flax grows a good length and fibre is produced which is the best raised in Russia and possibly second to none as regards quality and strength.

Although it is recognised as being the best practice to ret flax in water, there are many large areas where no water

suited to the purpose is available. The freshly deseeded straw is then spread thinly over the ground so as to allow alternate dew, sunshine and rain to carry the process of decomposition far enough to allow the fibre to be detached from the woody part of the straw. The very nature of this process, depending as it does upon favourable weather conditions, often gives rise to a product of very low quality: nevertheless, in many parts of Russia, this method of retting is the only one available and enormous quantities of "dew-retted" flax are annually prepared.

Following a crop or two of rye, oats or barley, flax is often raised year after year on the same land until the soil becomes so impoverished that scarcely anything will grow on it. The land is then allowed to lie fallow during a number of years, after which the scrub is burnt off and the process repeated on the freshly broken land.

Better conditions prevail in the western provinces, especially in the Baltic Provinces of Livonia and Esthonia, a territory which came under Russian authority at the beginning of the eighteenth century. These provinces are inhabited principally by Letts, who like the Esthes of Esthonia are in reality Finns and are people possessing some energy and determination. The usual practice among farmers in those districts is to autumn plough, then sow winter grain and in the spring to sow and harrow in the best grain. As a rule, the peasant grows what he requires regardless of all other considerations; consequently the rotation adopted depends less upon his knowledge of matters agricultural than upon his personal requirements. Only on the larger estates—apparently those over a hundred acres—is any regular course of rotation adopted; judging from numerous inquiries the following is accounted the best practice—fallow, rye and clover, barley, flax, oats and fallow.

There is a growing belief that agricultural progress will depend not so much on an increase in the acreage under cultivation as on improvements in method being effected; the feeling after progress noticeable in the Baltic Provinces receives considerable stimulation from the strong German and British community of business people in Riga.

The good harvests of the last two or three years have put many of the small farmers in a position to purchase modern implements and at present there is a large demand for iron ploughs, small winnowing machines and harvesting machines. The importation

of agricultural machinery has increased enormously during quite recent times. At some of the posting stations and local trading centres a fair assortment of modern implements may be seen and small machines of British, German and American manufacture. German and American goods sell more readily than British, not because of any superiority in quality or workmanship but simply because the German manufacturer ascertains what is required and sends it, whilst the Englishman sends what he is accustomed to make in the ordinary way regardless of any particular local requirement.

Much of the western country is covered by forests which extend as great arms across the land. Crops of potatoes, barley, flax and oats occupy small patches of the open country. Animal manure is very scarce, the soil is hungry and until the financial position of the peasant farmers has been improved by several more relatively good harvests they will not be able to afford the outlay necessary on livestock or to purchase artificial fertilisers.

Leaving Livonia and travelling eastward, the conditions become more truly Russian. There is indeed some excuse for the pride the Letts exhibit in speaking of a journey into the next province—Pskov—as a journey into Russia. The country loses in interest, there is less land under forest, the trees are smaller and there is less cultivated land. The plots of arable land resemble remote patches in a great garment.

In Russia proper the standard generally is lower than in the Baltic Provinces, agriculture is more primitive, resembling that of the north. Farming is extensively conducted on the triennial system—winter grain, summer grain and fallow—although the more intelligent adopt a six years' rotation, which includes potatoes, flax, clover, oats, barley and fallow.

In the vicinity of the city of Pskov are two brothers, energetic men, who have farmed a small property of their own during many years past and it is interesting to note that both admit that they are perfectly satisfied and pleased with their crops. There is one feature of their farming which is not often met with and which is of particular interest to flax growers who insist on the need of a change of seed every year. These two men always carefully select sufficient of their best crops to furnish seed for sowing in the following year. They have grown flax and other crops from the same strain of seed in this manner

during the last twenty years and their crops to-day are superior to others in the district. This is not to be regarded as instancing an improvement in the quality of flax seed for fibre production but as showing how deterioration may be prevented. That deterioration has taken place is beyond question and is admitted by Russian farmers themselves. Generally speaking, 1 acre of land at the present time yields 2 cwt. of finished flax fibre; twenty-five years ago the yield was $3\frac{1}{2}$ cwt.—a loss of more than £2 per acre to the peasant producer. Most countries, if not all, depend upon Russia either directly or indirectly for their supply of flax seed, so it is not surprising to hear universal complaints about the decreasing yield of fibre from the flax crops.

In the west central provinces, the number of horses and cattle kept by the peasantry is very small. When a household does possess a cow, it becomes the duty of some old person or of a child to accompany the animal throughout the day as it goes browsing over waste places, so as to prevent it doing damage by wandering on to the unprotected fields. For similar reasons, little children are sent out with the geese to wander with them wherever they go and to bring them home again at dusk.

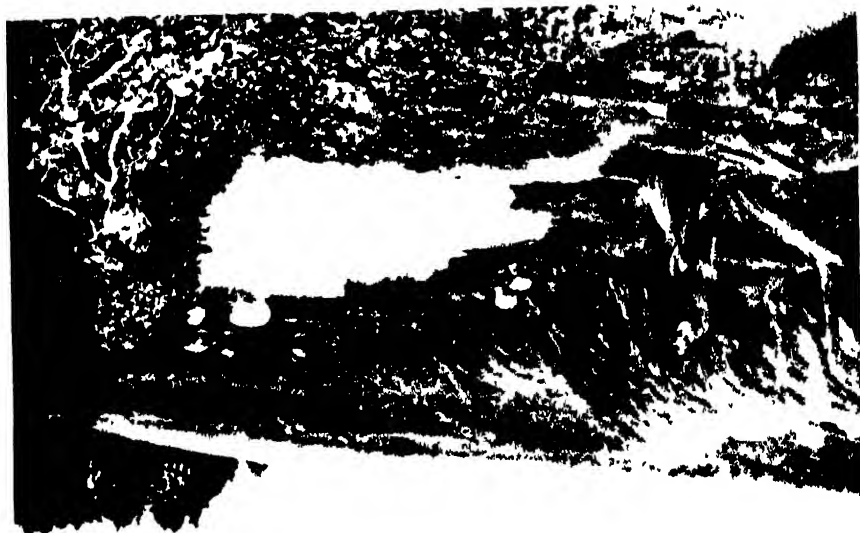
The governments of Pskov and the neighbouring governments of Livonia, Vitebsk, Smolensk and Tver constitute the most important flax-growing area in the world. It is no exaggeration to say that nearly the whole of the linen trade depends upon this great flax district. It is not surprising therefore to find that the keen cosmopolitan competition for flax fibre is waking up the slothful peasant and that the Ministry of Agriculture is endeavouring to improve present methods of preparing flax.

The general practice with this crop is to pull the plants before the seed has ripened and to tie them up into bundles, so that all the roots are at one end. The next operation is to remove the seed. Sometimes this is done in the field and the green stems are at once retted in water; or the pulled flax may be dried and then deprived of its seed. By whichever method the seed is obtained from the straw, it is finally dried artificially at a fairly high temperature and then spread on a stone floor to be threshed. Threshing often consists in a horse dragging a wooden roller about over the seed so as to crush the "bolls," the seed being separated from the chaff by repeatedly screening in a draughty situation.

Spreading Hay in the Province of Pskov



Komov, Lushkov, Ising, Pit



Most villages in Western Russia possess a common threshing-floor and a specially constructed drying house fitted with a fireplace, where the inhabitants can dry their crops. Not only is the final drying of the flax seed carried out in a heated chamber but grain crops in general are frequently so treated after having been dried as far as possible out-of-doors. This artificial drying operation often lasts for two or three days and, if the outdoor conditions are not favourable to drying, a longer period is necessary before the crop can be deprived of its moisture sufficiently.

Dotted about at convenient places all over this part of the country small pits may be seen in which water accumulates. At the proper season of the year, these are used as pits for retting flax. During early autumn, when the flax straw is taken from the water and is spread on the land, so as to complete the retting process, the whole countryside becomes covered with flax. One may drive many miles and see scarcely a change in the monotonous landscape ; everywhere flax, nothing but closely arranged rows of retted straw spread over the country.

Further south in the same province, near the upper part of the river Sheion and not far from Dedowiezy, is one of the three stations for the promotion and improvement of flax cultivation which have been established by the Ministry of Agriculture. At this station various methods of retting are practised and the application of artificial manure and the use of better appliances are explained and demonstrated to those who desire to become improved. Much rain falls in that district about harvest time and in consequence considerable difficulty is experienced in getting the crops up in proper condition. To overcome this difficulty and to make the farmer less dependent upon the weather, several drying sheds have been erected to receive the crops. These are simply constructed sheds with open sides, fitted with trellis shelves, so that the crop laid upon them is dried equally both from below and from above. Flax and clover dried in this manner are found to be superior to crops which have been dried in the open subject to the inclement weather. Clover dried under cover is beautifully sweet and fragrant and the fibre obtained from flax straw allowed to dry in the shed is of superior quality ; moreover the saving of good seed is made possible. So much success has attended the experiment that quite a number of drying sheds are now in process of construction.

Journeying in a south-easterly direction the scenery improves: instead of a vast almost treeless expanse, the country becomes undulating and trees are in plenty. Much of the land is covered by tall grass and silver birch trees grow in great profusion. Villages are even less frequently seen and the approach to them as well as their general appearance would prompt strangers to give them a wide berth. It is, however, worth while to seek a possible entrance, where the mud is shallow, so as to have an opportunity of partaking of peasant hospitality with one of the enterprising farmers of the district.

He will conduct his visitor to one of the log-built cottages which are bunched together about a wide muddy track—to one of larger size, perhaps, which besides a chimney boasts of some ornamental woodwork about the window frames and is situated close to several small sheds and an enclosure of apple trees. Mounting a few rickety steps, the cottage is entered by a door leading on to a small gangway alongside a central partition which separates the farmer's living quarters from those of his small collection of livestock—all under one roof.

Only a dim light prevails, just sufficient to make visible a small loft above the gangway where there is a stock of hay and straw, some baskets and a few sacks. Below, on the ground, a horse is seen standing on a scanty litter of straw between a pile of wood and the central partition of the cottage; poultry, pigs and maybe a calf will fill up the gaps between queer-looking carts, agricultural implements and a quantity of odds and ends. Leading from the gangway is a small room illuminated by means of a tiny pane of glass. In this little place, on a raised hearth, there is a cooking-stove of massive proportions, sundry cooking pots and earthenware utensils. The atmosphere is hot, stuffy with smoke and laden with various odours of animal and vegetable origin. From this apartment the farmer's dwelling proper is entered by a loosely hung door. It is a simply furnished abode containing a few chairs, some boxes, a table or two, several plants on a shelf before the window and a roughly fashioned cupboard in one of the corners.

The main features of the room are the stove and the bed, both in point of size and importance. The stove is a great brick and stone structure which is stoked from the little room outside. It is so built that part of its hot surface extends from the floor to the ceiling in each room and generally a long broad seat forms

part of the hot surface, so as to provide a comfortable couch during the winter. The broad bed is usually built in a recess between the stove and the central partition—certainly against the stove—and is separated from the room by a tall screen which is often pleasantly ornamented in a simple manner by some dexterous work with an axe. There will probably be an "ornament" under a glass shade occupying a place on a table and some damp garments hung over a cord drying by the stove. Sometimes as many as five *ikons* will be hung on the wall and at least one small lamp will throw a faint light upon their glittering surfaces.

Russians are kind hospitable folk and the simple farmer is not behind his richer countrymen in the matter of entertaining a guest, although the means at his disposal may be of the most primitive kind. There are few things they like better than manipulating the sizzling *samovar* and dispensing tea while the wife produces rye-bread, honey, fruit and as a particular luxury—some eggs. They offer all they have and sincerely hope it will be accepted. Their soft eyes beam with pleasure when they are sipping hot weak tea with a visitor at their little table. Sugar is seldom used, the tea being sweetened to taste by each person taking frequent mouthfuls of honey dug out from a big lump of honeycomb by means of a small spoon.

In this simple manner the peasant farmers live, cultivating flax and oats with which they trade and small quantities of rye, hemp, clover and potatoes for their own use. Here and there the Commune still survives, the village land, for which they are taxed as a community, being divided up according to the number of souls in the village at the time of division. This is done by the Village Commune or Council of Elders, who not only allot the ground to the inhabitants according to the working ability of the various households but strictly supervise its cultivation, deciding when to plough, when to sow, and when to reap. So the peasant has no personal interest in the land, he has only to carry out the communal instructions so as to avoid trouble with the Elders. He may neither increase nor decrease his agricultural task without the consent of the Commune, neither may he seek employment elsewhere without their permission: individual enterprise can find no place in a life conducted under such circumstances.

A fair proportion of the country is covered by pine, birch

and acacia trees, whilst further east, on towards the Valdai hills, extensive forests occupy much of the land. Frequently, when passing along the clearways through the forest, large clusters of acacia and birch trees may be seen growing amongst the pines. These are pointed to by the peasants as being places good for a habitation, as a patch of good land where they would like to live. Several of these coveted patches may be seen in process of preparation for farming : the trees having been felled, the scrub and roots are burnt out ; after this, the land is ploughed and probably the first crop sown will be flax. When the plot is ready the peasant either builds himself a log cottage on the spot or he removes one he may have elsewhere, transporting the structure a few logs at a time by means of a small cart.

There is some fine rough woodland country about the Valdai hills, wherein rise the small streams which unite at Selisharova to form the river Volga, which flows in a south-easterly direction to the town of Rshéf. With the exception of some slight differences in detail, it may be said that all small Russian towns are alike. They consist of an amazing collection of two-storey houses and shops, which are generally built of wood, situated some distance from a railway but close to a river. In the midst of the town will be a large church of pleasant outward appearance and close beside an open market place. The roadways and paths will be in a bad condition and everything appear to be in a state of disrepair.

The best day to visit Rshéf is on the Sabbath, market day, for then Rshéf is animated as well as muddy. Peasants come into the little town from distant parts, bringing with them all kinds of goods for sale. From soon after dawn until eight a.m., a steady stream of pedestrians and small V-shaped carts come down the main muddy street from the south and across the Volga by the pontoon bridge from the north and up the river bank, all going towards the market. Bags of grain and linseed, bales of flax and baskets of apples form the major part of the traffic but the merchandise exposed for sale on carts and on the ground includes cattle, pigs, poultry, clothing, pottery, apples, baskets, implements of wood, and other commodities. Merchants come from afar to buy grain and fibre : indeed at certain seasons of the year the competition is so great that agents go out to meet these small carts as they approach the town ; business is done at once and the sold goods are brought into Rshéf. As would

be expected, this anxiety on the part of dealers to purchase the peasants' produce is arousing in them rather a pronounced business propensity. Between half-past nine and ten o'clock all the little shops are closed and trade stops while a service is held in the church: afterwards the market proceeds until two p.m., when trade ceases for the day—one might almost say for the week. Rapidly the people leave the town, taking with them various articles purchased at the shops and salt from the barges on the river. Once more Rshéf becomes a quiet place: at night there is no light in the town and no sound to be heard except from the watchman who walks about the dark streets telling of his approach by swinging a noisy rattle and showing his whereabouts by a lantern.

There are flax dealers from all over Europe congregated in Rshéf. At one small house there are six men of different nationality living together; they converse in German and each man goes his own way, buying according to the instructions he receives by telegram. Nearly all day long and part of the night up to two o'clock telegrams arrive at that humble dwelling. The slamming of doors, the heavy tread of messengers up and down stairs and the word "telegram" all form part of the daily existence of these buyers in Rshéf.

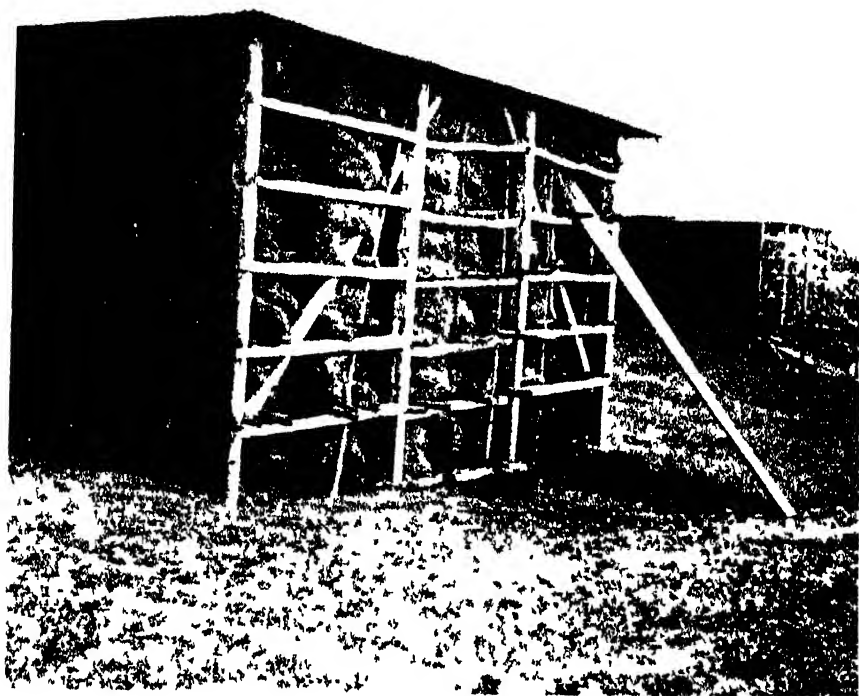
Much if not all of the peasant produce, be it grain or fibre, is very imperfectly cleaned. Their implements are primitive and they use them carelessly. But a change is coming; it is already noticeable in many places how mechanical devices are finding favour and that they will bring an improved condition. The Mayor of Rshéf has been inquiring for suitable machines of simple construction for cleaning flax, machines such as the peasants could purchase and take to their homes. He knows what is required and is seeking where he can procure machines suited to the purpose. The replies to his inquiries are really significant of the spirit in which trade is carried on with Russia. Those received from British firms read, "We do not make such machines"; the replies from German firms read, "We will make the machines you require." With this difference of attitude in mind, it is not difficult to understand why British goods are being steadily ousted from the Russian market. It avails little to gaze in wonderment at our ever-decreasing imports into Russia when the fault lies with us for not studying the conditions of Russian trade.

Between Rshaf and Moscow there are extensive pine forests, girt about and intermingled with beautiful groups of silver birch and thorny acacia trees. The country is slightly undulating and terminal moraines form quite a feature of the district. Considerable quantities of apples are grown and cattle are to be seen in great numbers, presumably because of the market for meat and dairy produce afforded by Moscow. Apart from this cause, the Government and the Provincial Councils have done much to foster and develop this side of farming.

The remarkable and elegant city of Moscow, of which all Russians are justly proud, possesses a great number of educational institutions. One of the most important is the large Agricultural College, which is situated amid delightful surroundings in a beech wood some little distance from the city. The College is well attended and although the building and the laboratories are extensive, so great is the bustle and stir that the place seems to be overcrowded.

Eastward from Moscow a great featureless country is passed through, where neither hedgerow nor tree breaks the monotony of a desolate plain. Generally speaking the soil is light; it blows about as dust during the dry summer months and after rain makes very disagreeable mud. The ways of communication are far worse than those found in the western provinces; there are few railways, and scarcely any roads. Irregular tracks connect a village with its neighbourhood and may be seen as a pair of wavy lines stretching across the country. Except in the villages situated nearer to Moscow, the conditions under which the peasantry live are extraordinarily low. In the more remote parts poverty is to be seen on all sides, misery being written everywhere and it is shocking to behold the conditions under which some of the peasants exist.

The severity of the Russian winter is keenly felt by the inhabitants of this flat unprotected region, where cold, searching wind and snow sweep unmercifully across the plain. It is not until the end of March that the snow begins to melt; with the advent of April warmer winds rid the earth of the last snow and bring forth vegetation with exceptional rapidity. Cattle which have survived the seven months' trial—poor starved beasts!—are driven to the grazing land and it is small wonder that the release from winter is celebrated by a religious ceremony. About the middle of April the land is prepared for summer



Dry



From Hupnuc to the Steppes

grain; that is to say it is shallow ploughed and the seed sown. Seldom is the soil enriched by the addition of manure, because there is little available; moreover, it not infrequently happens that towards the end of winter, when the stock of fuel is exhausted, part of the thatching from the roof, as well as the manure that has been saved, is burned in the stove to keep the cottage warm.

Agricultural work in connexion with summer grain proceeds during six or seven weeks following St. George's Day and during that time, as well as throughout the open season, the peasant labours on the extensive grain-raising lands of the land-owner, in lieu of paying him rent for the ground he works for himself. When the spring sowing is completed the fallow land is ploughed up and made ready for autumn sowing; this takes until about the end of June. Following this comes haymaking and harvest commences about the middle of July, lasting until the end of August. Hay is mown by small scythes and the standing crops of grain are cut by reaping-hooks. Men reap and women and children twist the bands and tie the crop into small sheaves, which are subsequently carted to the village threshing-floor, where the grain is removed in the old style by means of the flail. During September winter grain is sown and provision is made for the oncoming winter.

Into this programme of events there must be read the celebration of religious feasts and saints' days, all of which take time. Russian peasants are not contented with fifty-two Sabbaths during the year; they celebrate some 150 holy days in addition and so great is their love of idleness that besides keeping the holy days of their own village they will frequently leave work and go to the celebration of a saint's day in a neighbouring place. This means that much of the available time during the open months of the year is devoted to religious idleness.

Although the eastern provinces are primarily a grain-producing district, some considerable quantity of flax is grown in the north-east in the neighbourhood of Viatka and the organisation of co-operative societies in the district beside the Volga between Yaroslavl and Kazan has made it possible for the small farmers to carry on dairy farming profitably and to export butter and large quantities of eggs. It may not be known generally that about half the eggs imported into Great Britain come from Russia, some thousand million annually.

The high road of Russia is the Volga, a vast traffic being carried upon the slow-moving, turbid river. It is perhaps owing to that traffic that better agricultural conditions obtain in the Volga region; with increasing herds of cattle, agriculture is advancing and the conditions are becoming more stable. Deeper ploughing, the use of iron ploughs and grain drills are all making for better harvests.

The most important town in East Russia is Samara, the centre of the greatest grain-producing district in Europe. Day and night loads of wheat, oats and barley arrive there from the remote parts of the vast cultivated area surrounding the town. There is great activity in the docks and warehouses; barge-loads of grain are towed up the river for exportation from St. Petersburg and Riga.

A large proportion of the Russian-grown tobacco comes from the province of Samara but the quality of the product is not very good.

It is instructive to visit these more remote regions, to see how great is the area of land already cultivated and the almost equally great area not yet opened up to crops. The harvest is enormous because of the greatness of the area occupied, not because of large yields. As a rule the crop is small; expressed in bushels per acre the average is:

Winter wheat .	14	Winter rye .	12	Oats .	11½
Spring wheat .	11½	Spring rye .	11	Barley .	11

When these figures are compared with the following data recording harvests from some of the Rothamsted plots it will be seen how closely they approach the yield from unmanured land:

	Unmanured.	Dung.	Complete artificial.
Average for five years .	12	36	39 bushels of wheat

Already Russia exports more wheat than the United States, so that when better methods of agriculture find place and when some few more successful seasons enable the farmers to purchase machinery and artificial manure, it is probable that Russia will be able to meet all the European requirements in the way of grain.

The severity of the winter is not felt by the young corn, because the deep snow which covers the land protects the crops from wind and frost. It is surprising to find that the

more hardy crop, winter oats, does not seem to be grown, nor is there any information to be had as to the reason.

In remote districts where little beside grain is cultivated it is almost impossible to picture what the effect of crop failure must mean; the distress must be awful. Even at the present time the terrible calamity of six years ago is still felt by the peasantry, many of whom sacrificed all their belongings and pawned their future labour in the struggle against starvation. So poor were the crops of that year (1906) that only about one-half of the grain sown was recovered at harvest. There was nothing wherewith to pay taxes and nothing to live upon during the winter and no reserve stock of grain in the district; thousands of people and cattle died from starvation.

Nearer to the Ural Mountains the country is far more picturesque; it is undulating and well wooded, resembling pleasant downland, affording a welcome contrast to the dreary flat district to the immediate west. In the neighbourhood of Ufa and Orenburg cattle-rearing has become quite an important business and here again the organisation of co-operative societies has proved a great benefit to the small farmers by enabling them to export large quantities of butter. Villages are few and far between; indeed, in some parts, there seems to be no population at all, though it is said there are about forty inhabitants to the square mile.

To the west the broad Volga flows slowly towards the Caspian Sea, passing through richer soil than in its northern course; but apart from this very noticeable improvement, there is little to be seen which is different from other districts. Besides cultivating wheat and other grain, horses, cattle and sheep are extensively bred and as might be expected agriculture is not conducted on such poverty-stricken lines. There are quite a number of private estates where up-to-date farming is practised; some of them are of tremendous extent, embracing many thousand acres of land under wheat; horses and sheep are bred on an equally large scale.

Continuing in a south-westerly direction, the renowned Steppe region is reached, one might say the boundless Steppe, because this rich band of soil stretches from the Carpathian Mountains in the west far away eastward into Siberia; in fact, it is not quite known how far it does extend. The European portion is a vast undulating plain, mostly covered by sweet

herbage, where there is not a tree to be seen and where droves of horses roam about in almost a wild state. A journey across this region resembles a sea voyage; the lines of the horizon constantly retreat before the eyes without changing in aspect: occasionally the view extends far away into the distance where earth and sky merge together into an indefinite haze. Not a tree is to be seen, scarcely a bush of respectable size to give a touch of variety to the landscape. Although the soil is rich, it is exceedingly light, lighter even than fine sand, so that one's own conveyance raises in its wake a cloud of dark dust which slowly drifts across the country.

Villages are more frequently met with than in other parts of Russia; they are cleaner and generally more orderly. As no wood is available, the cottages are built of brick and stone and are heavily thatched with straw: quite a contrast to the rickety wooden structures which constitute a village in the forest region. The climate is almost temperate, the soil dark—nearly black—and very deep, producing good crops of grain. There must be a wonderful future in store for this fertile area. The condition of agriculture in the Steppe region is advanced when compared with other parts of Russia; already the peasants have grasped the advantage of using machinery and through the operation of credit associations they are now able to purchase modern appliances. The Russian peasants are not thrifty, they would seldom save sufficient to be able to purchase a machine outright, so these associations will probably play an important part in developing Russian agriculture. In many villages modern agricultural appliances are to be seen amid primitive surroundings and during the month of August, when harvest is in progress, the changing hum of the steam threshing machine may be heard on most of the large estates. The corn is cut and left in the field until threshing commences, when a long stream of carts carry the sheaves from the Steppe to the threshing machine. Numbers of women and girls receive them, cut the bands and pass the sheaves on to men who feed them into the machine while others stoke the engine with the issuing straw. When threshing commences, it is often carried right through to completion, lasting day and night for several weeks on the large estates, great animation prevailing; indeed it is a wonderful and picturesque sight.

Extensive horse breeding is a feature of the north Steppe

region: in one province, that of Voronezh, there are no less than 230 breeding studs, and more than 370 studs in the adjoining provinces of Tamboff and Orel. Towards the town of Orel there is a Government Agricultural School where lads from the surrounding villages may go to receive practical instruction in farming. This establishment is managed on good lines; the pupils are not taken from their humble surroundings and placed in circumstances far in advance of that of their homes. They live together, under proper supervision, in a commodious building and they keep house for themselves, taking it in turn to cook and to clean. They are shown how to make use of the material at hand, be it indoors or out, how to construct farm carts, wheels, tubs and so forth, so that when they return home they become improvers instead of grumbling talkers who cannot do anything for want of the appliances upon which they have been taught to depend. Dairy work, pig-breeding, poultry-farming, smithery and harness-making all form parts of the course of instruction.

In some places agriculture is mainly carried on by the womenfolk, the reason being that their household is capable of cultivating more land than is at their disposal, so the men go away to the towns and seek employment at hotels or practise a handicraft while the women carry on the farming operations. One of the most difficult in the north Steppe region is the management of the hemp crop, which, like flax, requires much judgment and labour and for this reason, although large quantities of hemp are raised in Russia, it is a crop which is generally grown in small plots.

In Russia hemp is grown both for seed and for fibre, necessitating a separate treatment for the male plants and for the female plants. The male plants come first to maturity and are cut or pulled as soon as the stems show signs of changing colour; the female plants, which grow to a greater height, are left standing for the seed to develop. At a later period, when the seed is almost ripe these plants are also cut and after properly drying them the seed is pulled off. Generally speaking the male plants are spread on the ground and allowed to rot by the action of the dew. The female plants yield a much coarser fibre and are submitted to a water retting process similar to the treatment of flax. Rectangular pits are dug in the black earth in the vicinity of a stream, so that water will accumulate there and bundles of the

female stems are packed so as to occupy only the central portion of the pit, leaving a free water space surrounding the hemp. A little straw is scattered over the top and clods of earth are stacked on the straw, so as to sink the hemp below the surface of the water. When properly retted the bundles are withdrawn and the stems spread out on the land to dry, the separation of the fibre from the retted stems being carried on during the winter.

Besides hemp and the usual crops of grain, there is quite a large quantity of tobacco grown on the rich dark soil of this district, especially in the provinces of Tamboff, Poltava and Tchernigoff, over 25,000 tons being produced annually in the last named province. The quality of this tobacco crop is held to be very superior to that grown in the neighbourhood of Samara. Further north, at Orel, a busy little town about a night's journey south of Moscow, the Government have started an establishment where the cultivation of hemp may be studied. They have also installed quite an instructive exhibit of all types of machinery required in hemp cleaning and the manufacture of rope and twine. The exhibit comprises both simple and complicated machines and they are fitted up so that anybody can receive instruction in working them. Every inducement is being used to encourage people to work up the raw fibre instead of exporting it and to improve the methods of cultivation; but as the poorest class of peasantry is concerned with hemp cultivation it is difficult to effect any improvement. Those in charge of this station are certainly firm believers in the use of machinery for everything and enthusiastically point out the superiority of the British-made goods. They would like to have more of the smaller machines of the same high-class workmanship but find that the British firms expect to have a large order placed with them at once and are not willing to invite new business by supplying small items.

The district known as the "Pale" comprises most of the south-western provinces extending from the Baltic Province of Courland to the west shore of the Sea of Azov. Nearly 95 per cent. of the Russian Jewish population live within this area but only few occupy themselves with agriculture. The Jews are bad farmers and generally lack inclination to take part in agriculture except by dealing with the produce. It is a significant fact that nearly all Russian dealers are Jews; in fact nearly 97 per cent. of the grain dealers in the south-west provinces are of that race.

In Poland and the north part of the "Pale," large quantities of potatoes, apples and sugar-beet are grown in addition to the more usual crops of grain. Further south, besides sugar-beet, rye and wheat, maize is extensively cultivated; tobacco growing is largely carried on in the province of Bessarabia. Flax is extensively grown as a seed crop in the southern part of the Steppe region where the climate is warm. For the most part, agricultural practices differ little from those which obtain in similar regions, with the exception that farming is more intensive and machinery plays an important part in all operations.

Owing to a number of distinct causes, such as better education, mineral resources and the requirements of local industry, the extreme south and the Caucasian provinces boast of still better conditions of agriculture. In the Caucasus, Russian husbandry is seen at its best; wheat, rye, sunflower, melons, fruit, tobacco, tea and cotton are all raised in the district between the Black Sea and the Caspian Sea. The horrors of famine are unknown in this beautiful region because of the diversity of the crops, as well as the steadying effect of horse-breeding; cattle and small-stock raising allows of intensive cultivation being carried on.

The time will come when these more flourishing conditions will extend over a large part of Russia instead of being confined to a relatively small region; indeed it is admitted that a great change is setting in; already there is evidence of this even in the more remote parts of the Empire. Left to themselves the peasants will not change but show them how to progress and they will progress up to the hilt. At the present time, it may be said truthfully that they are being shown how to progress.

The undoubted desire of the peasant is to become an independent agriculturist, to own his own land; to this end, assistance is being given by the operation of the State Land Fund and the Peasant Land Bank, which jointly work to bring about the change. In recent years the State has done much to improve the condition of the agriculturist, recognising in a practical manner the valuable constructive work done by co-operative societies. The possibilities that have been opened up and the progress that has been made in agricultural districts by the organisation of co-operative and credit societies are quite remarkable. Judging from the present beneficial results, it would seem that the Ministry of Agriculture looks well to the future when fostering the growth of these institutions.

THE STRUCTURE OF METALS

THE INFLUENCE OF MECHANICAL TREATMENT ON STRUCTURE

By CECIL H. DESCH, D.Sc., Ph.D.

THE microscopic structures described in the former article¹ were those of cast metals and of worked metals which had been sufficiently annealed to cancel the effects produced by the mechanical treatment to which they had been subjected. The mechanical treatment, such as forging, rolling, pressing or wire-drawing, to which metals are usually subjected influences in a most important manner the microscopic structure as well as the mechanical properties of the metal; as numerous relationships between these properties and the structure have been established, the examination of worked metals is a highly important branch of the metallographer's activity. The subject offers a wide field for future research, on account of the diversity of mechanical conditions that come under consideration and the minute and elusive character of some of the internal structural changes to which they give rise.

One of the chief factors in determining structure is the temperature at which the change of form of a metal by mechanical means, such as rolling, is conducted. A mass of metal that is forged or rolled at a bright red heat and then allowed to cool slowly assumes a structure which is essentially that corresponding with the annealed condition; it may differ in several respects from that of the same metal as cast, the difference being in the arrangement of the micrographic constituents, however, not in their nature or proportions. More rapid cooling may, of course, disturb this equilibrium, as in the case of a cast alloy. On the other hand, when the rolling or forging is carried out at a considerably lower temperature, readjustment of the crystalline structure may be impossible; the cold metal then exhibits unmistakable evidence of the treatment to which it has been

¹ SCIENCE PROGRESS, No. 25, p. 87.

subjected. The effect of hot-rolling on steel has been referred to in the former article (photograph 5), where it was shown that the grains of iron and the areas of pearlite are elongated in the direction of rolling. Such flow-structures are of frequent occurrence in rolled metals, so that the direction which a specimen originally occupied in the rod or plate from which it was cut is readily determined by microscopical examination. Naturally, enclosures of slag or sulphide and similar impurities are also elongated in the direction of rolling when the temperature is so high that they are in a liquid or plastic state at the time.

A metal which has only been worked while hot has properties which differ but little from those which the metal possesses in a fully annealed state; it differs from a cast metal in being more compact and generally more uniformly crystallised but the elastic properties are not greatly modified, except in so far as they depend on the crystallisation.

The effect of mechanical work on the properties of a metal becomes more pronounced as the temperature falls. So long as the temperature of working is above a certain limit, different in the case of each metal and alloy, internal strain is removed as fast as it is produced by a process of recrystallisation whereby the equilibrium is re-established. At lower temperatures this is not the case: the properties of the metal undergo more or less permanent alteration, until at the ordinary temperature nearly all metals are very appreciably "hardened" by the process of hammering, pressing, rolling or drawing into wire. The term "hardening" here denotes a change in many properties which are closely associated with one another. The actual mineralogical hardness—that is, the resistance to scratching—as a rule is little affected but the elasticity is increased and the ductility diminished, whilst the electrical conductivity is also lessened and important changes are produced in the electro-chemical and thermo-electric properties. Such a metal is said to have been "cold-worked," although the temperature of working may be considerably above the atmospheric temperature provided that it is below that at which recrystallisation occurs freely. The crystals of such a metal as copper or 70:30 brass are crushed and deformed, the extent of the deformation naturally varying with the degree of cold-working, whilst the structure of alloys containing two or more micrographic constituents becomes extremely confused and small areas of an eutectic may be

entirely indistinguishable. The detection of impurities by means of the microscope is therefore far more difficult in worked than in cast or annealed specimens. The effect on a homogeneous alloy is well seen on etched surfaces of rolled sheet brass. Still more severe distortion is seen in hard-drawn wires, in spun sheet metal and in cold-pressed objects such as cartridge cases.

The facts which have to be explained in the mechanical deformation of metals are the plastic yielding of the crystal grains, which distinguishes a metal from a material such as sandstone—the grains of which are usually separated by pressure before any great deformation of the stone as a whole is produced—and the remarkable increase of hardness which is the consequence of the cold-working of most metals. The two properties, plastic yielding and increase of hardness, are intimately connected but it is only in quite recent years that either of them has been satisfactorily explained and several points still remain obscure. Both properties depend on the minute internal structure of the crystals.

Viewed in the gross, there is considerable analogy between the behaviour of crystalline and of amorphous materials under a mechanical stress sufficient to produce deformation. Thus, if a rectangular block with polished surfaces be compressed either uniformly over one face or locally by means of a knife-edge, systems of lines appear on the remaining faces and these lines have the same general form and direction whether the material examined be wax, hard gelatin or metal. The arrangement of lines can be calculated mathematically and is independent of the nature of the material. The differences between amorphous and crystalline materials become obvious whenever the deformation is studied more minutely. Whilst a fracture in an amorphous substance may occur in any direction indifferently, a crystalline substance has definite planes of weakness along which rupture takes place by preference. Moreover, an amorphous substance may undergo considerable permanent change of shape without the development of any fracture, however minute, provided only that sufficient time be allowed for the deforming force to exert its effect. Examples of this are seen in the slow sagging of glass tubes supported only at the ends and in the remarkable experiments with brittle cobbler's wax which have been made familiar by Lord Kelvin.

Time also plays a part in the deformation of the softer metals but the mechanism of the process is quite different. The bending of a stick of sealing-wax, for example, is not accompanied by any obvious change in microscopic appearance; but even in the case of the softest metals, such as lead, the structure is altered. Lead, in fact, is a convenient metal for the study of the process. A smooth surface is prepared and the metal is deformed, say by lightly bending between the fingers. Examination under the microscope shows at once that a change has taken place, the originally smooth surface being crossed by very numerous lines arranged in parallel groups; unlike the systems of lines common to amorphous and crystalline materials, to which reference has been made above, these systems of microscopic lines do not bear any necessary relation to the direction of the deforming force. On the other hand, they are very evidently related to the crystalline structure. Fig. 1 represents a surface of lead after bending; it will be seen that a close parallelism is preserved by the lines in each crystal but that their direction changes abruptly from one crystal to its neighbour.

The lines thus developed have been termed "slip-bands" by Ewing and Rosenhain¹ and the name has been generally adopted. They are parallel with the cleavages of the metallic crystals and their direction in any one grain seen under the microscope depends on the crystalline orientation of that grain. It has been found possible to show, by the direct examination of a cross-section, after protecting the marked surface by depositing a thick layer of copper on it by electrolysis, that each line is really a minute step and that the surface on one side of a line is at a different level from that on the other.² The same conclusion may be reached by illuminating the specimen obliquely and rotating the stage of the microscope; it is then obvious that the lines disappear in certain positions and flash out again on reaching such a position that they reflect the incident beam into the tube of the microscope. As the lines in any one grain flash out simultaneously whilst they are independent of those in neighbouring grains, their dependence on orientation is clear.

These facts furnish the explanation of slip-bands. They are

¹ *Phil. Trans.* 1889, 193 A, 353.

² W. Rosenhain, *Journ. Iron and Steel Inst.* 1906, ii, 189.

due to the slipping of certain portions of the crystal over others along the planes of weakness or cleavage planes. When the stress in a crystal grain becomes too great for the metal to yield elastically, slipping along these planes takes place and the shape of the grain is changed not continuously as that of a truly plastic substance would be but by a series of dislocations completely resembling in origin and appearance the "step-faults" of the geologist. After the formation of the slip-bands, provided that no further change take place, the internal structure of the crystal is not affected, since the displacement of neighbouring portions of a crystal is only one of translation. Hence, if we examine a polished surface on which slip-bands are obvious and then remove a thin surface layer by grinding and expose the crystalline structure by etching, the slip-bands do not reappear. This fact distinguishes them from the grosser changes of structure which are produced by mechanical means under certain conditions and especially from twinning. Twinning planes reappear after removal of the surface and re-etching and are easily recognised when once the manner in which they differ from slip-bands has been appreciated. Both twinned lamellæ and slip-bands may be present in the same crystal, but whereas the latter are universal in metals after cold-working, the former are less frequent and are only developed abundantly in certain classes of metals and alloys, of which austenitic steels (such as manganese steel) and copper and its α -alloys, including yellow brass, are familiar examples. The strained surface of lead in fig. 1 shows twinning lamellæ as well as slip-bands. Fig. 2 represents a cube of ingot iron after compression; both slip-bands and crystal boundaries are distinguishable, the latter having been made visible by the strain without any etching process.

To produce slip-bands it is not necessary that the stress applied should exceed the elastic limit of the specimen. A crystalline metal, even if practically free from impurities, is not a homogenous substance but is built up of distinct grains aggregated to form a mass. When a stress is applied, it is impossible that it should influence every grain equally and it may readily happen that a few individual grains are stressed by an amount exceeding the elastic limit whilst their neighbours are under a much lower stress. Every slip along a cleavage plane brings about a redistribution of stress, tending to make



Fig. 1



Fig. 2

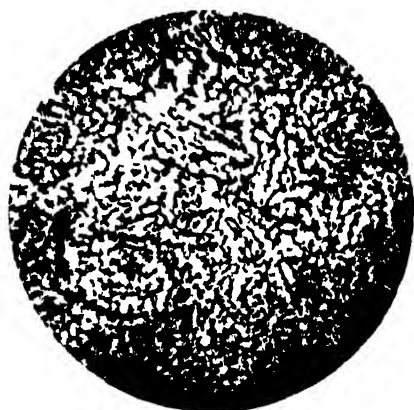


Fig. 3



Fig. 4

further slipping unnecessary unless the stress be increased. In the case of progressively increasing stress, more and more crystal grains are dislocated in turn and the constantly varying direction of the local stresses causes the opening up of new cleavages, so that a grain examined microscopically shows two or more intersecting systems of slip-bands corresponding in direction with its systems of cleavage planes.

If this were all that happened, the hardening effect would remain unaccounted for, as a mere translation of crystal elements does not cause a change of properties. As a matter of fact a more profound structural change occurs as soon as the amount of cold-working is considerable. The slip-bands lose their simple character and become broad and prominent on a smooth surface. Etching no longer removes them completely; a close examination proves that the surfaces along which slipping took place are now separated by a layer of material which differs in some way, both chemically and physically, from the unaltered crystals.

An explanation of the hardening of metals has been given by Dr. G. T. Beilby,¹ who has based his conclusions on observations of the effects produced by polishing. Whenever a metal is subjected to friction a superficial layer is formed which possesses peculiar physical and chemical properties, being hard, isotropic and more active chemically than the original metal. A similar layer may be formed in the interior of a metal by cold-working. The first motion of translation along a gliding plane may produce little effect but by repeated rubbing a layer of the hard material is built up between the two surfaces which hinders further slipping; the process being repeated on successive cleavage planes, eventually the whole mass of the metal is appreciably hardened.

Hardening by cold-working cannot be continued indefinitely but reaches a limit, which has a definite value in the case of that particular metal under given conditions. Further stress weakens the metal by causing rupture of the hard layer and consequent separation of adjoining crystals. The effect is often seen in hard-drawn wire. If the drawing be continued too long the wire loses its strength; if a longitudinal section be examined, it is seen that only the outer shell is continuous,

¹ *Phil. Mag.* 1904 [vi.], 8, 258; *J. Inst. Metals*, 1911, 6, 5.

whilst the inner core is broken into cylindrical fragments with conical ends separated by distinct cavities.

It might be thought that this result would only be attained when the whole of the metal had been converted into the hard material but this is not the case. When a wire that is hard-drawn as far as possible is examined, it is apparent that the greater part is still composed of the original crystalline metal but that the crystal grains have been reduced in size by crushing and that each small grain is enclosed in a hard shell of the modified material. Further slipping along cleavage planes is hindered or prevented by this comparatively unyielding, brittle casing. When a section of such a hardened rod or wire is etched, the shell or casing is dissolved more readily than the crystalline core, so that the structure becomes visible.

One of the most characteristic properties of the hard modification produced by strain is its power of flowing. Thus in the hard-drawn wire it envelops the unchanged cores, filling the intercrystalline spaces without a break. This property is most conveniently studied in the surface films produced by polishing. Whilst the grinding of a metal surface with emery or similar abrasives is simply a process of cutting, innumerable fine grooves being produced, the subsequent process of polishing with alumina or rouge is of a totally different character. Dr. Beilby has shown that even in the case of such brittle metals as bismuth or antimony the surface layer flows like a viscous liquid under such treatment. The grooves are partly smoothed out by removal of the intervening matter and partly filled up or bridged over. Etching removes the altered film; scratches which had merely been bridged over during polishing reappear on etching. This reappearance of "latent" scratches has long been familiar to those who have examined etched sections. Measurements made on polished crystals of calcite by an ingenious chemical method show that the thickness of the surface layer of modified material is of the order of 500-1000 $\mu\mu$. A pattern once developed in an alloy by etching may be obliterated by polishing, in which case the gradual disappearance of the structure as the altered material flows into the hollows may be followed with great ease.

It is observations of this kind that have led to the conception of the hardened modification of cold-worked and polished metals as an undercooled liquid of high viscosity.

This view is perfectly consistent with a high degree of brittleness. Cobbler's wax is a typical example of a substance which flows like a viscous liquid but yet is brittle under a suddenly applied stress; the combination of these two properties, at first sight contradictory, is not uncommon. The amorphous, isotropic character of the hard modification is fully in accordance with such a view, which is further supported by considerations of the following kind.

A modification which stands to the ordinary crystallised metal in the relation of an undercooled liquid must be unstable at all temperatures below the melting point. At the ordinary temperature it is related to the crystalline metal as glass is to the mixture of crystallised silicates which is formed from it when it devitrifies or as vitreous silica is to quartz. It may thus be expected to show, relatively to the crystals of the same metal, a lower density and a greater activity towards solvents and to exhibit a tendency to crystallisation whenever the circumstances are favourable. These expectations are fulfilled. A cold-worked metal is actually of somewhat lower density than one that is fully annealed, although the difference is small, as is natural in view of the fact that the conversion always remains incomplete. The greater sensitiveness of hardened metals to attack by chemical agents has already been mentioned and is confirmed by determinations of electrolytic potential, which show that a highly worked metal always becomes the anode when coupled in an electrolyte with a piece of the same metal in an annealed condition.

The tendency to return to the crystalline form is also well marked. The change takes place with extreme slowness at the ordinary temperature but much more rapidly when the temperature is raised. At a certain point, termed the "crystallisation temperature" by Dr. Beilby, the return takes place suddenly; the progress of annealing may be followed by means of tests of elasticity or still more conveniently by determinations of the thermo-electric difference between the specimen and one of fully annealed metal.

The tendency to recrystallise must be present in all cold-worked metals even at atmospheric temperatures, although greatly restrained by the internal viscosity. It usually becomes evident, however, even under such unfavourable conditions, to a sufficient extent to constitute a serious difficulty in technical

practice. The "season-cracks" which develop in brass are due to differences of stress existing in the inner and outer layers of worked brass objects and are the outcome of the process of spontaneous recrystallisation. Still more remarkable examples are seen in objects of brass or German silver which have been subjected to very severe cold-working in the shape of "spinning" or pressing between dies. In thin articles such as brass lamp-reservoirs numerous cracks are apt to develop which gradually involve complete disintegration of the metal. This change proceeds more quickly in a warm than in a cold atmosphere; it has been described by Prof. Cohen¹ as "strain-disease," owing to a remarkable similarity to the now well-known "tin plague" which occurs in cold countries. The tin plague is due to the change of ordinary white tin, which is unstable below 18°, into grey tin and is propagated by contact with articles of grey tin. So also the recrystallisation of severely strained metal is accelerated by contact with the stable crystalline modification. In some of the experiments a design was etched on a sheet of metal in order to expose the crystalline structure by removing the superficial fluxed layer and the clean surface was then placed in close contact with another sheet of the same metal in a cold-worked condition; in the course of one or two days, at a temperature of 100° or upwards, the design was found to have been transferred to the second sheet, the unstable modification on the surface having reverted to the stable crystalline form.

The view was and frequently still is held by engineers and others that a metal in use, especially when the load which it carries varies in direction or intensity, tends to become more coarsely crystalline; in fact, failures of structures under stress are very commonly attributed to crystallisation of the metal. Growth of crystals takes place readily at high temperatures, to such an extent that iron bars forming part of a furnace exposed during several years to a temperature favourable to crystallisation have sometimes been found, when the furnace has been dismantled, to consist of only two or three large crystals. At somewhat lower temperatures vibration has been found to favour this process by facilitating the rearrangement of the solid particles when the metal was initially in a condition not that of equilibrium but there is no evidence that a thermally stable

¹ E. Cohen and K. Inouye, *Zeitsch. physikal. Chem.* 1910, **71**, 301.

metal undergoes any appreciable spontaneous change of the kind at atmospheric temperatures, whether assisted by vibration or not. There is some little evidence that vibration favours the return of an unstable alloy to the stable state at the ordinary temperatures but so far this case has not received much attention from the practical point of view.

The evidence for the popular opinion as to the influence of fatigue on metals rests entirely on the appearance of the fractured surface. The appearance of fractures is constantly used in practice as a means of judging of the coarseness of grain of a metal and very useful results are obtained in skilled hands from the comparison of specimens broken under precisely similar conditions, although the accuracy of the method is naturally less than that of microscopical examination. On the other hand, a single piece of metal may give two entirely different types of fracture if broken in two different ways, as by slow tension and by sudden shock. A metal which breaks with a so-called "fibrous" fracture in an ordinary testing machine may have a coarsely crystalline fracture when broken by shock or by fatigue.

The manner in which fracture actually occurs has been studied in detail by methods involving the fatigue of the metal. For instance, a rectangular rod of steel may be fixed at one end to a revolving shaft, whilst the other end is loaded by a weight suspended by means of a stirrup passing over a polished sleeve. The rod is thus subjected to a bending stress which varies periodically in direction. By polishing and etching one surface of the bar and interrupting the test at intervals, the course of destruction of the specimen may be followed with the microscope.¹ The development of slip-bands begins in a few crystals and gradually spreads to others, whilst at the same time new systems of lines appear in the grains which were first affected. As the alternations of stress are continued, the lines broaden, indicating the formation of a layer of amorphous material of appreciable thickness along the rubbing surfaces; after a time actual cracks become perceptible. The cracks always pass through the amorphous films, not between the crystals (that is, in such materials as soft steel, from which brittle inter-crystalline eutectics are absent). A crack once started tends to spread by localisation of stress at its ends but only a few of the cracks which appear reach any great development, the

¹ J. A. Ewing and J. C. W. Humfrey, *Phil. Trans.* 1902, 200 A, 241.

others being arrested by meeting the crystal boundaries. When it happens that the directions of the slip-bands in two adjacent grains nearly coincide, it is possible for a crack to be propagated and as every increase in its length produces a further concentration of stress, a crack once extended over several grains tends to spread to the exclusion of neighbouring smaller cracks. Ultimately the crack spreads through the whole rod with increasing velocity, owing to the increasing intensification of local stress.

The "crystalline" fracture of metals broken by fatigue is thus accounted for. The glistening facets which are usually regarded as crystal faces are in reality cleavage planes exposed by the process just described. When produced by simple alternations of stress, such a fracture is not accompanied by any marked deformation of crystal grains whilst a fracture produced by slowly applied tensile or bending stress preceded by great deformation has an entirely different character, the crystals being drawn out and torn rather than snapped asunder. In the well-known instance of wrought iron, the presence of brittle slag bands causes fissility in one direction, so producing the characteristic "fibrous" fracture.

The brittleness occasionally exhibited by masses of mild steel, such as boiler plates, is not revealed by the usual tests involving the slow and continued application of stress. It is possible for a metal to show the required strength and ductility in a tensile test and yet to be so brittle that a sudden blow will break it without previous yielding. In order to guard against such accidents, a special form of test is required in which the application of the stress is such as to cause fracture in the manner just described, that is, by rupture of single crystals along their cleavage planes. Such tests are of two kinds, the one involving repeated alternations of stress and the other a suddenly applied shock—both kinds are susceptible of many different modifications. An alternating stress test may consist in bending the test-piece to and fro or in alternately stretching and compressing it, whilst a shock test may be made in a variety of ways, by means of a falling weight, a swinging pendulum or a revolving arm. The test-piece intended to be broken by shock is generally notched to localise the stress. Although considerable differences of opinion exist as to the most suitable form of test, it is certain that either of those mentioned gives a more

accurate indication of the presence or absence of brittleness in a specimen of steel than the ordinary tensile test.

Brittleness in steel may be due to the presence of impurities, principally phosphorus, which has a remarkable effect in coarsening the structure and developing the cleavages. The influence of these dangerous elements is thoroughly well understood and the control of metals by chemical analysis is largely designed to guard against danger from this source. Other less well understood factors remain, among them the influence of nitrogen, to which some authorities have attributed the brittleness which occasionally develops in mild steel plates with age. This is a point which has not yet been fully investigated, although certain remarkable changes of structure, including the production of large and conspicuous cleavages, have been recognised as associated with the presence of nitrogen, which is apparently retained by the iron in the form of a homogeneously distributed nitride. Apart from these chemical conditions, the principal factor which determines the toughness or brittleness of a given steel is the size of grain and this is in turn dependent on the thermal treatment. Considering first a steel containing only a small proportion of carbon, heating to a high temperature within the austenite range, say to 1200° or 1300° , produces a coarse structure, the size of the grain being approximately proportional to the temperature and this coarseness is retained after cooling to the ordinary temperature. In fact, the size of grain is a function of the maximum temperature to which the steel has been exposed, provided that no mechanical work has been applied. If, on the other hand, the metal be rolled or forged while hot, the mechanical treatment breaks up the crystal grains while it continues and the final size of grain is a function of the "finishing" temperature and not of the maximum temperature. Coarse crystallisation due to overheating is thus obliterated by work, provided always that the metal has not been "burnt," in which case the grains, separated by films of oxide, do not reunite during cooling. Steels very low in carbon also become coarse and brittle if annealed for a long time at a low temperature, the growth of the grains being extremely rapid somewhat above 700° , as was shown in the previous article.

Steel which has been overheated or which has been annealed for too long a period at a low temperature may be restored to

a normal condition by heating until the austenitic region is entered and then cooling. A fine grain is obtained in this way and dangerously brittle steel, if not burnt, may thus be made equal in quality to steel which has not been rendered coarse at any time. The minimum temperature for the purpose varies from 950° for very mild steels to 800° for hard steels.

When the proportion of carbon is somewhat greater, so that the pearlite forms a considerable fraction of the entire mass, a second factor enters, namely the condition of distribution of the carbide. Rail steel containing about 0.45 per cent. of carbon may be taken as an example. A slowly cooled or annealed rail contains its carbide in the form of laminated pearlite. Prolonged annealing not only causes an increase in the size of the grains but if it be conducted at a temperature below that at which the carbide is absorbed, it has the further effect of causing segregation of the carbide, a final state of equilibrium being reached only when the whole of the carbide has been gathered into isolated masses which lie between the grains of ferrite. Such a condition is eminently favourable to brittleness, on account of the facility with which the cleavages opened in one grain can be propagated. It has been found that the maximum toughness is obtained when the steel is cooled so rapidly that the carbide, instead of forming parallel laminæ, remains in a minutely granular state in the condition known as sorbite. Such a condition may be obtained by a process of semi-chilling, in which the cooling is not sufficiently rapid to harden the steel but is too rapid to allow the carbide to segregate.

Similar considerations apply to other metals and alloys. Heating to a high temperature increases the size of the grains, whilst hot-working destroys the large crystals, so that a fine-grained structure may be obtained by selecting a suitable finishing temperature. Fig. 3 represents a transverse section cut from a rod of Muntz metal, which has been rolled hot and has a fine grain, the α and β constituents being arranged very uniformly, with little or no tendency to rectilinear groupings. Fig. 4 represents a rod of the same alloy heated to 850° and slowly cooled without applying work. The magnification is the same in both cases. It is obvious that the size of grain has increased enormously, whilst the α -crystals also show a strong tendency to assume rectilinear forms and to become arranged parallel with the cleavages of the β -crystals from which they have separated.

Such an alloy is far more brittle than the rolled specimen. If, besides heating it to a high maximum temperature, the alloy be quenched, so that it is entirely in the β -condition, the brittleness is enormously increased, the β -grains being separated by rectilinear boundaries without any α -constituent to produce even a partial union.

The mechanical behaviour of metals at high temperatures also has great technical importance. Such objects as the valves for the admission and regulation of superheated steam or the plates and stays of locomotive fire boxes are exposed to severe mechanical stress at temperatures very considerably above that of the atmosphere. It is well known to engineers that all metals deteriorate in strength as the temperature increases but satisfactory information on the subject is curiously scanty. Generally speaking, the tensile strength, both of pure metals and alloys, diminishes as the temperature rises. The ductility of a cast or annealed metal also diminishes at first, whilst that of a cold-worked metal increases, owing to progressive annealing. At higher temperatures the ductility varies in an apparently capricious manner, finally reaching zero at or near the melting point. A number of factors are evidently concerned in the form of the ductility curve and much work will be needed in order to disentangle them.

The most satisfactory experiments of this kind are those recently conducted at Liverpool by Mr. G. D. Bengough.¹ Considering only the tensile stress under which a specimen breaks, it appears from these tests that the stress falls as the temperature is raised in a manner which is best represented by two intersecting lines one of which is straight whilst the other may be either straight or curved. The general condition presented by pure metals or homogeneous alloys is shown in fig. 5.

The line ABC represents the variation of strength with the temperature of a cold-worked metal, whilst DBC represents that of the same metal in a cast or hot-worked condition. The change of direction at B is always well marked in the actual curves. The point B is designated by Mr. Bengough the "temperature of complete recuperation." The curve AB is evidently a range within which annealing of the cold-worked metal, that is, recrystallisation of the amorphous modification, is going on. It is suggested that our ordinary cast or hot-

¹ *Journ. Inst. Metals*, 1912, 7, 123.

worked metals contain a proportion, perhaps small, of the amorphous material and that this accounts for the form of the curve DB. On this view, a specimen composed exclusively of crystalline material would exhibit a strictly linear change of strength with temperature, as shown by the line EC. Beyond B all three curves coincide; B is therefore the highest temperature at which the amorphous modification can exist. This limit lies at about 650° in the case of copper, 395° in that of aluminium and at 710° in that of a homogeneous alloy containing 80 per cent. of copper and 20 per cent. of nickel.

The hypothesis is ingenious but the continued existence of the amorphous modification at such high temperatures is contrary to the evidence of experiments on the elasticity and thermo-electric power of worked metals, which indicate lower

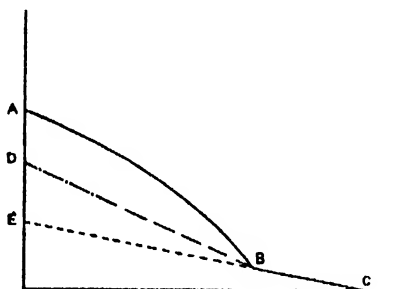


Fig. 5.

recrystallisation temperatures, about 250° for copper. The sharpness of the break in the curve at B does not serve to suggest that the point is merely the upper limit of a crystallisation which sets in with great rapidity at a temperature 400° lower and there are other difficulties which need further elucidation.

In spite of the closeness with which the hypothesis of an amorphous modification fits the facts, it has not met with universal acceptance. The view also finds favour¹ that continued cold-working involves merely a greater and greater development of slip-bands, so that the individual crystalline masses which remain unchanged in form become smaller and smaller but without the appearance of any new form of material.

¹ O. Faust and G. Tammann, *Zeitsch. physikal. Chem.* 1910, **75**, 108.

Prof. Tammann has recently applied this hypothesis in detail to the explanation of the properties of hardened metals. He rejects the assumption of an unstable amorphous state on several grounds, of which the principal are the absence of any permanent alteration in a metal when the pressure applied is equal in all directions and the fact that cold-working generally produces a slight diminution of density, whilst the application of an increased pressure might be expected to lead to the formation of a denser rather than of a lighter modification. The diminution of density is attributed to the formation of minute gaps between different lamellæ when the amount of slipping and shearing becomes large. It is supposed that the energy expended in causing slipping is stored in the crystal and that thin lamellæ are constantly tending to reunite to form larger crystals; the greater energy-content of the hardened metal and its tendency to return to the normal condition of coarse crystallisation are due to the same cause.

The diminished electrical conductivity of a metal which has been hardened by drawing into wire is readily explained on the hypothesis of an amorphous modification. If this be rejected, the diminution must be attributed partly to internal rupture of the material and partly to the effect of orientation by sliding, it being assumed that the conductivity of a metal is greater in the direction perpendicular to the principal cleavage than parallel to it. There are obvious difficulties in the way of such an explanation but it is supported by experiments which show that drawn wires recover their conductivity on annealing though severely twisted wires are rendered permanently worse conductors. In the first case the effect is due to reorientation of the crystals and a rise of temperature, by allowing freer play to the capillary forces, brings about recrystallisation, whilst in the twisted wire reorientation cannot account for the lessened conductivity, which must be due to cracks and therefore does not disappear on annealing. The argument does not appear to be conclusive and the writer prefers the hypothesis of an amorphous material, especially in consideration of the microscopical evidence from polished and unpolished metals which is not discussed by Prof. Tammann.

The theory of thin lamellæ in metals, the number of which

¹ *Zeitsch. Elektrochem.* 1912, 18, 584.

is increased by cold-working, whilst subsequent annealing produces reunion under the action of capillary forces, was proposed as long ago as 1868 by Prof. G. Quincke,¹ who has since modified his views and now assumes that every metal, even when pure, has a heterogeneous foam structure² and that the cell-walls, which are chemically different from their contents, modify the influence of mechanical work in displacing the lamellæ. In other respects his explanation of the phenomena is in agreement with that of Prof. Tammann.

At the other extreme stands a remarkable hypothesis which has been proposed recently as the outcome of important experiments in the Geophysical Laboratory of Washington.³ The hypothesis is to the effect that the flow of metals is due to an actual melting. It is true that increase of hydrostatic pressure has the effect of raising the melting-point of all but a very few metals but it is contended that pressure producing flow must have an entirely different effect. It has in fact been shown, on theoretical grounds, that pressure must always lower the melting-point if it be applied in such a way as to act only on the solid while the liquid is free to escape;⁴ the conclusion has been verified in the case of ice. If the heat of fusion and the density of a metal at its melting-point are known, it is possible to calculate the pressure which would be necessary to melt a metal at atmospheric temperature. This has been done and although the numbers obtained are very large, the author of the memoir does not regard them as impossibly so. The order in which the metals appear in such a list coincides exactly with that of their elastic properties, showing that the relation between melting-point and elasticity is a real one whether the actual form of relation proposed be correct or not. It is difficult to picture the manner in which such melting can take place. It is true that the pressure between two portions of metal on opposite sides of a cleavage plane may be very much greater than the average pressure on the metal under stress but the pressures demanded are very large (1760 atmospheres in the case of lead and 14,000 atmospheres in that of silver at 27°, for example) and it would

¹ *Ber. Akad. Berlin*, 1868, 132.

² *Proc. Roy. Soc.* 1906, 78 A, 60.

³ J. Johnston, *J. Amer. Chem. Soc.* 1912, 34, 788.

⁴ J. H. Poynting, *Phil. Mag.* 1887 [v.], 12, 32.

appear to be impossible that such pressures could be confined to the solid crystals.

The importance of a microscopical control of engineering and structural materials will be obvious, even from this hasty and incomplete sketch of the relation between structure and properties. Whilst ultimate chemical analysis is of great importance in controlling materials, there is much necessary information that cannot be obtained by such means. Proximate chemical analysis, which in some cases affords valuable information, is almost in its infancy. Its absence is in a large measure supplied by microscopical analysis, which permits a visual separation of constituents. The highly important question of crystalline arrangement within the metal is only to be approached by microscopical means and although the complete correlation of structure with mechanical properties may be only an ideal towards which workers in metallography are striving, the knowledge already available on this subject suffices to make the microscope an indispensable auxiliary of the balance and the testing machine in metallurgical work.

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THE PLANET MARS

PART II

By JAMES H. WORTHINGTON

IN the preceding article, I have explained the precautions that are taken in observing this planet and have drawn attention to various considerations which justify students of its features in attaching reality to their observations, as well as in feeling assured of the correctness of the arguments which they venture to use.

The account is not complete nor can it be, as the subject is one that is being developed almost daily; but sufficient has been said to illustrate the methods peculiar to the investigation.

The appearance of Mars in the telescope at Flagstaff, when conditions are favourable and due precautions are taken to stop down the instrument and to insert appropriate dark glasses, is a most surprising revelation. The telescope presents us with a disc of about five times the apparent diameter of the full moon as seen by the naked eye: brilliantly lighted, it shines with well-defined, delicately tinted patches of colour.

The snow cap is seen at the pole. Farther down the disc, areas appear of a greenish-blue colour in which is visible a wealth of minute stippled detail—too fine to be called features but coarse enough to produce the impression of variation in texture. These green areas are very clearly defined at their edges and the better they are seen, the more clear-cut do they appear to be. In addition to the green areas, there are ruddy ochreous stretches extending over five-eighths of the surface of the planet.

Thus far nothing new or startling is seen. But when, during a few brief moments, the definition becomes perfect—and such moments are infrequent—an amazing network of very fine lines, arranged criss-cross-wise in perfect geometric fashion, is apparent. These lines occur in all latitudes, alike over green and

ochreous areas ; they are the "canals" of which so much has been heard.

In seeking an explanation of the general appearance of the planet we may recall first that it is amply proved by spectroscopic study that the known chemical elements are the common property of the visible universe. With a few exceptions, perhaps, all the elements that exist in the stars are to be found on the earth and *vice versa*. We therefore need have no hesitation in drawing on terrestrial experience when investigating Mars, which differs from the earth mainly in being more distant from the sun and of smaller mass. These two differences alone suffice to explain most of the contrasts that are evident on comparing the surface of Mars with that of our earth.

Again the kinetic theory of gases provides a criterion by which we may judge of the probability of the presence of an atmosphere and its possible nature. According to this theory, the molecules of all gases, at any given temperature, move with velocities characteristic of each gas ; though the molecules of a given gas move with varying velocities, both the mean and the maximum speeds are functions of its molecular weight.

The power of a celestial body to retain a gaseous atmosphere about itself depends at any given temperature upon the force of gravity at its surface, the which force is a function of its size and mass. This gravitational force is capable of controlling and retaining particles or molecules which move with a speed less than that which would be attained by a particle falling from infinity to the surface of the planet under consideration ; this velocity, for the sake of brevity, is called the critical velocity for the planet because particles moving faster than this, in the right direction, must inevitably fly off the planet and escape into space.

If it can be shown that the molecules of a given gas at the surface of a planet would move with a maximum velocity higher than the critical for a given planet, the conclusion is inevitable that the planet cannot have permanently an atmosphere composed of this gas.

An example or two will make the operation of this law clearer. It has been found that the sun possesses an atmosphere largely composed of hydrogen and this is in harmony with the fact that the critical velocity at the sun's surface is something over three hundred miles a second, whereas the maximum velocity of

hydrogen molecules there is probably about 50 miles a second. The earth has a lower critical velocity, namely 6·9 miles a second, whilst the maximum velocity of the hydrogen molecules at the mean temperature of the air would be about 7·4 miles per second and but little hydrogen is found free in our atmosphere. The critical velocity at the surface of Mars is about 3·1 miles per second and the temperature, as we shall see, is probably not so much below that of the earth as to make it likely that gaseous hydrogen is a constituent of its atmosphere though other gases whose maximum molecular velocities are less than this may well be present.

On account of the weakness of gravity on Mars it is probable that though water may be scarce, yet the commoner constituents of the earth's atmosphere whose molecular velocities at its surface are all likely to be less than 3·1 miles per second may well be common. Among these gases are those which make life possible here—namely water vapour, oxygen, nitrogen and carbon dioxide. We need, therefore, feel no surprise when appearances on Mars indicate the presence of gases which are thus shown to be theoretically possible. That there are other causes besides gravitational weakness operating to rob the planet of a terrestrial atmosphere cannot be doubted: diminished pressure of sunlight is perhaps the most obvious. It appears therefore that we are justified in concluding that the atmosphere of Mars may be like our own, though less dense and that probably it is disappearing gradually. It will be seen later that this conclusion is amply corroborated by the detailed observations of the surface features and their changes.

Our estimate of the temperature at the surface of Mars is based upon the following considerations. The heating and lighting power of the sun at the distance of Mars is about half what it is on the earth; but only about 40 per cent. of the solar heat which the earth intercepts ever reaches the surface; the remaining 60 per cent. is thrown back into space by our atmosphere. On Mars the conditions are very different. Though the planet only receives 50 per cent. of the earth's share, it retains a much greater proportion, for the low albedo or reflecting power of Mars is an indication that more than 80 per cent. of the incident light is retained and hence it appears that the surface of the planet receives from

the sun at least as much as falls upon the surface of our earth.

Now the average temperature of the surface of the earth is about 60° F. It seems probable that Mars should not be much colder. No doubt the thinness of the atmosphere of the planet will have a chilling effect but it seems certain that the conditions are such that winter and summer, frost and thaw, as well as vital changes like those which occur on our earth, are to be expected.

The low reflecting power of the planet itself is also evidence that the atmosphere is scanty, though the strong whitish glare on the limbs which there obliterates surface detail is clearly seen in the middle of the disc and proves that there is an atmosphere.

I come now to speak in greater detail of the markings of the disc and the changes they undergo.

On Mars there are, roughly speaking, six different kinds of markings—viz.:

- Greenish areas ;

- Ochreous areas ;

- White areas near the poles ;

- White areas which behave differently in the equatorial regions ;

- A network of extremely fine lines called "canali" ;

- Small round dark spots forming knots in the network of the "canali."

After duly noting the changes in which all these features share, I shall attempt to outline an hypothesis which will consistently account for all of them simultaneously.

The first features to be considered are the white patches which, in their respective winters, are so conspicuous at the poles. For many years it has been noticed that these polar patches are smallest at the time when the summer heating of the pole is greatest—a time corresponding to late July in the earth's Northern Hemisphere—and that the maximum extent of white occurs at midwinter. This in itself is an indication that the material of which the caps are composed may be water. As has been shown above, the temperature at the planet's surface is such as to justify this view. I shall therefore assume that the white patches consist of water and pass on to examine other observations which have been made of their behaviour.

The polar patch vanishes in the increasing heat of spring. The blue strip which surrounds it is fluid, for it has been found to polarise light and is exactly the colour of water. The blue strip clings to the dwindling cap, just as pools form around melting snow. When all the white is gone, a dark smudge as of wet ground is seen in its place. By this time, the blue stuff has also disappeared—has either flowed away or evaporated.

A recent discovery speaks accurately as to the temperature prevailing round the pole in the later part of the summer, for there appears at this time in the subpolar regions of the planet, on the sunrise edge of the disc, a whitish patch which has the unique property of being fixed. It is on the surface of the planet but does not partake of its rotation. It is therefore a state through which the surface passes at this particular hour of the morning. There can be no doubt that it is hoar-frost. It may seem surprising that this should be visible but the appearance is so striking as to show obviously and unmistakably on many photographs of the planet.

When I first saw the patch I was so struck with its appearance that I sought for evidence as to whether it had been seen prior to the announcement of the discovery of its nature. I have found many notes of white patches being observed in the appropriate position the meaning of which was not divined at the time. I need scarcely say that this discovery was made at Flagstaff, where also the majority of the data used in this paper were obtained.

Lowell has shown that this morning hoar-frost appears exactly where it should do in the coldest part of the autumn hemisphere, which is obviously not the pole but the place where the increasing nights have become long enough to cause the land to lose more heat than it receives daily from the sun.

The presence on Mars of water in all its three states being indicated, it is natural to inquire what happens to it when it leaves the pole. Most of the greenish areas lie in a belt about the south temperate zone. When the snow at the South Pole begins to melt, this zone of green proceeds to darken, the wave of colour beginning in the southernmost part and gradually spreading northwards. That this change may be due to vegetation is evident. All circumstances are propitious. There is sufficient heat. Water is present to nourish it. And all we know of the Martian atmosphere points to

its being one that could support plant growth. The colour of the green areas is that of vegetation and the change to green occurs at the right season. In the other hemisphere the green areas, being in the grip of winter, are pale and faint. This also is to be expected.

Granting that the water from the pole has moved down the disc, it is natural to ask how it makes the journey. Accurate measures made at Flagstaff prove that the shape of the planet is such that fluids on its surface are in static equilibrium and that water therefore could not flow naturally down the parallels as it manifestly does. The conclusion is that it is transported by some artificial means. We are thus led to seek for evidence of artificial water channels. These the "canali" supply. For the "canali" develop down the disc equatorwards, their colour deepening ahead of the green areas through which they run, thus proving that the water reaches the regions through which they pass before it arrives in the surrounding regions. The lines which we see are presumably not the water channels merely but the strips of country irrigated by them. The rapidity with which the water progresses is indicated by the growth of the strips and proof is obtained in this way that the development of vegetation is not due to the perennial sunshine but to the seasonal irrigation.

Wherever two canali cross, a minute dark spot or oasis comes into view and in no other part of the planet do these dark spots develop. The general appearance of the canals must be noted. They all are perfectly direct and uniform in their course; many are more than 1,000 miles long, one, the *Eumenides Orcus*, being upwards of 3,000. The larger canals are all arcs of great circles and are therefore the shortest possible courses between the points they unite. Many are double, thin components being rigidly parallel, though not always equal in intensity. All are uniform in width throughout their course, though the width is individually characteristic of each canal—some being strong lines, which are probably 30 miles wide, whilst the fainter lines are the merest gossamer threads, visible with the greatest difficulty and probably not more than a mile wide, perhaps less.

Lowell's experiments on the visibility of distant telegraph wires have shown that lines of this width should be visible.

In all cases the width, which is itself imperceptible, is estimated by the intensity. That there is a range of 30,000 per cent. in the intensity proves that the larger canals are nowhere near the limit of vision—a conclusion amply verified by the fact that many of the larger ones are clearly visible on photographs of the planet. All these canals are equally geometrical in their appearance and it is inconceivable to those who see them that they are anything but the work of intellect. They are just what our study of the planet's conditions have led us to expect.

The existence of vegetation on Mars depends upon them and conversely it is evident that the vegetation, for whose production they were made, is a necessity to their makers. It would be natural to suppose that on a planet capable only of seasonal change due to water from either pole, each pole would nourish its own hemisphere. That this is not so, is another proof of artificial causes. For the vegetation caused by Southern water spreads far into the opposite hemisphere—defying all the laws of dynamics and symmetry.

Of the ochreous regions of the planet nothing has been said. There is little to say, in fact, as they are as changeless as the Sahara. In these regions only the parts near the poles show any change and this is of a doubtful nature. The conclusion seems inevitable that they are deserts. Their extent is most telling—for they indicate that the planet has advanced far in its course towards death and are evidence of that scarcity of water which is the sign of advancing age in planets.

On the earth the same process of desiccation is going on. There are two belts of desert. The most marked is the northern one. Wherever there is land this desertism shows. The Sahara, Arabia, Persia, Northern India and the Chinese desert in the Old World and the deserts of Mexico and the western part of the United States form links in this chain. Historical and geological evidence points to the fact that the belt is ever widening.

On Mars things have gone much further. Indeed water can only reach the more genial parts of the planet by means of the gigantic canal system which we are led to conclude has been made there in self-defence by intelligence.

On earth the cry of humanity is for bread and the great areas under wheat are perhaps the only changes which man

has brought into existence upon the earth's surface which are big enough to be visible to instruments of the same order as ours from a distance such as that at which Mars is situated. On Mars, as on our earth, presumably the principal necessity is water and it is the means employed there to bring water into operation that has proved to us the existence of the intelligence which wants it.

Like friends in need the two planets may become acquainted through their necessities.

The canali are there and it must be admitted that they have been made. It is therefore of interest to inquire what are the difficulties which have been overcome. In like work on earth, the chief difficulty is the mountainous nature of the surface, which renders world-wide canalisation almost inconceivable.

The first thing that strikes the observer of Mars is the flatness of the surface. No mountainous markings have ever been seen and yet if there were any they should be visible on the terminator at sunrise or sunset by the shadow they would cast. A hill 2,000 feet high would be quite visibly indicated in this way. We are therefore warranted in saying that there are none as big as this. Irregularities have indeed been noticed on the terminator but they are only explicable as high clouds or in some cases effects of contrast and irradiation due to differences of colouring of the surface. Incidentally the flatness of the planet's surface, which so clearly makes artificial canals easy of construction, renders untenable one of the many theories which have attempted to explain them as natural phenomena—volcanic cracks in fact akin to those which radiate from many of the larger craters on the moon.

On Mars there are no such craters and yet on the moon the craters are more conspicuous than the cracks—except at the time of the full—when the lighting is more favourable to the one than the other. Besides all this the lunar cracks are not straight and the canals of Mars are. Now there is every reason to suppose that the moon and Mars are made of somewhat similar materials.

If both have cracked, there is no obvious reason why one should crack crookedly and the other straightly. But whatever we think of the method by which these two globes (which

are not widely different in size) came into existence, it is clear from Darwin's Tidal Theory that the moon is a fragment broken away from Mother Earth, whereas Mars is an independent planet.

It is very probable that the moon owes its volcanic features to its terrestrial origin. Mars shows none of these rugged characteristics. If the planet had ever possessed mountains, these should be there still—for if we assume the green areas at present containing vegetation to be the beds of departed oceans, it is clear that like the moon Mars probably never possessed water enough to wash the mountains away.

Lowell has calculated that if the particles of which Mars is composed had fallen together under gravity the generated heat of mass would probably be less than that of molten iron—a temperature too low to cause much volcanic action. The case is further emphasised by the fact that if the planet grew gradually, it would be radiating heat and cooling layer by layer as it grew.

Another suggestion will illustrate the quandary in which those are placed who attempt to explain the obviously artificial canal by other means. It has been suggested that the canals might be in the nature of scars left by meteorites grazing the surface. Apart from the fact that meteors would require special training to produce any such effect, the moon again helps, for it is open to the attack of more meteorites than Mars, being nearer the sun, about which they all revolve: yet no such canalisation is visible on her surface. With the single exception of the valley of the Alps, no lunar feature suggests this origin—and further if the valley of the Alps be due to this cause, its appearance shows that the effect is quite different from any Martian marking, for it is at one end an ill-defined scratch and in the middle a deep furrow.

To return to the canal builders. They have had no mountains to contend with. Further, the force of gravity, which limits work on earth, is less potent on Mars, being only about 40 per cent. what it is on earth. The same muscular effort would accomplish two and a half times as much work in a day against it. But though we may feel sure of the existence of intellect on Mars, we know nothing and need not trouble much about its physical embodiment. It is quite evident that the physical difficulties have been overcome.

One of them is directly deducible and throws an interesting light on the nature of the water channels. Assuming that the Martian atmosphere exerts a pressure of $2\frac{1}{2}$ inches of mercury upon the surface—and it can scarcely be greater than this—Lowell has shown that water could boil at a temperature of 111° F. As the solar energy falling on Mars is certainly not much less than that which heats the rocks of the Sahara to at least 130° F., it is clear that evaporation is much more rapid there than here; and consequently water travelling in an open channel would evaporate long before it reached the tropics of the planet, a journey which we know occupies several weeks. It is therefore probable that the water is carried in something akin to pipes and this is rendered the more plausible by the fact that the water does not flow naturally but is driven, a conclusion to which the shape of the planet has led us.

No apology is made for this last speculation. It is, I think, directly justified by the observations and this one example serves to illustrate the amount of detail which is possible in constructing a picture of the happenings on the planet. Changes speaking eloquently of activity are to be found among the double canals, for they are not always double. The doubling is seasonal in its nature but not entirely so, for there are canals which sometimes double at the appropriate season and sometimes do not. That when they are not double their *alter ego* is lying fallow is strongly suggested.

Instances of this kind might be greatly multiplied but space does not permit.

There is yet another class of surface marking to be dealt with—namely, the white spots which are seen in the equatorial regions. They are intensely brilliant, often glistening but they seem not to be snow, for they are often most conspicuous in the height of the Martian summer; and it has been noticed above that they are not glaciated mountain tops. It seems natural to surmise that they may be beds of salt left by the evaporated seas. Their close association with the green areas strongly suggests this explanation. As yet observational data are too scanty to afford a firm base for conjecture but their increase of brightness under a high sun forcibly suggests a mineral origin. Further it may be remarked that vegetation would not invade them but would probably be near them at the bottom of the old marine depressions. A like instance on earth occurs

in the Egyptian oasis of the Fayûm, where the intensely salt waters of the Birket el Kerûm are within a few yards of some of the most fertile land on earth.

To sum up :

Recent investigation of Mars has revealed to us a world of great beauty but filled with signs of age, for it has evidently reached that apocalyptic period when there is no more sea. We have found reason to believe in the existence of a highly developed and intelligent race making a last stand against the increasing deserts of its world. The canals are evidences of tremendous and united efforts to eke out the decreasing water supply to the last drop. In this struggle we see, in some sense, a forecast of what the earth also must come to in the fullness of time.

We set out to learn about another planet. In return we learn much of our own and incidentally our eyes are opened to the demonstration of a truth long held by instinct, that we are not alone in the cosmos—that other worlds beyond the earth are no longer the dreams of fantastic poetry but firmly established facts of observational science. We see how the law of evolution which has shaped us to fit our surroundings has fitted other creatures in another world to cope with their special needs.

The falling apple led Newton to the law of gravity on the moon. In the same way the appearance of sprouting vegetation has led us step by step to recognise the law of evolution on Mars—a world where, as on earth but with differences, winter and summer, frost and snow, seedtime and harvest-time continue so long as there is water to support them.

No doubt the differences between Mars and the earth may have led the thinkers on the former planet to be sure that no intelligent being could exist on the earth owing to the recking wet and perennial clouds which enwrap it. But probably by this time they too have abandoned the puerile and absurd idea that *they* inhabit the only world where intelligent life is possible. It is well at least for us to realise not merely Man's place in the universe but that of Mars also.

THEORIES AND PROBLEMS OF CANCER

PART III

By CHARLES WALKER, D.Sc., M.R.C.S., L.R.C.P.

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HAVING considered prevailing views of the nature of cancer and the experimental work carried out in connexion with them, it is now possible to draw general conclusions. The most probable explanation of the behaviour of the cells of which malignant growths consist is that owing to the operation of some stimulus these are no longer subject to the co-ordinating influence which, under normal conditions, regulates the relations between the different groups of cells forming the body; the result is that the cancer cells live parasitically upon the organism.

Experimental work shows that the only way in which cancer can be transferred from individual to individual is by transplanting living cancer cells but to be successful the transplantation must be effected in animals of the same species; it is most easy in the case of animals of the same race or breed and is more difficult in proportion to the distance of relationship even within the same species.

The parasitic theory—the theory that the disease is due to a specific micro-organism—appears to be incompatible with many of the well-known facts connected with cancer; though many micro-organisms have been found in malignant growths, it is evident that none of those described up to the present time is found in all cancers and not in any other condition.

It is now necessary to deal with the present state of knowledge as to definite causes of cancer. To put the case briefly, cancer is known to follow upon prolonged and more or less continuous irritation and inflammation. It appears that in cases in which the irritation and consequent inflammation is slight, it must be continued during years before cancer develops. Chimney sweeps' cancer appears to be due to the creases in the skin being filled habitually with carbon; minute particles of carbon make their way between and even into the cells and cause a certain amount of cell proliferation, which, in

time, results in cancer. Persons who work with X-ray apparatus have in some cases developed cancer in parts of the body which have been continuously irritated and inflamed for years by the action of the rays. The chronic inflammation accompanying syphilis is regarded by many, probably with reason, as often resulting in cancer.

There is overwhelming evidence that cancer is commonly incident to several different occupations and habits in all of which chronic inflammation of some part of the body is involved, cancer occurring in the part affected.

That cancer should follow upon prolonged inflammation is compatible with the view that the cells have passed out of somatic co-ordination. Apparently all the somatic or body cells are destined to disintegrate within a limited space of time. In some groups of cells—those forming the skin for instance—the multiplication goes on actively throughout the life of the organism; in other groups, multiplication either does not take place or is rare in the adult. Chronic inflammation causes the groups of cells affected to multiply more than they would under normal conditions. It seems probable that the powers of normal proliferation of any given group of cells included in the body are limited and that when a certain number of cell generations have been produced the offspring tend to escape from somatic co-ordination as a stage on the way towards fertilisation. Having passed out of somatic co-ordination, the cells possess novel properties and, as the experimental work already described shows, are able to grow and multiply in a suitable environment just like the cells of grafts or cuttings of plants. The suitable environment is the body of the animal in which they arose or a body similar to it; and they live in it as separate individuals in a parasitic manner.

In every case in which a generally accepted cause of the disease is apparent the cancer is external, that is upon or near the surface of the body. It is quite likely that chronic inflammation is the cause of internal cancer also and various suggestions have been made on these lines. Chronic alcoholism resulting in inflammation and the production of scar tissue in the liver might well condition cancer, as also might chronic inflammation of the lining of the stomach. Primary cancer of the liver is very rare, however. Cancer of the stomach is common in men. Ulceration of the stomach is commonest

in young women but the ulcers occur usually away from the openings into and out of the stomach, while in men ulceration usually occurs near the opening at which the food leaves the stomach. Ulcers in the latter position are probably more subject to continual irritation, which may account for cancer of the stomach being common in men though it is rare in young women. It is possible that diet, in the broad sense, may have some connexion in these cases with the occurrence of cancer but it is going much too far to suggest, as has been done,¹ that cancer is due to food and drink taken at a high temperature and to the free use of wine, beer, spirits, flesh, coffee, tea and tobacco. We may, I think, dismiss most of these from among common causes of cancer. All the generally accepted causes of external cancer involve irritation which is more or less continuous and considerable in degree; all are probably sufficient to give rise to some local lesion and to keep up and increase this lesion when it has once been established. Food and drink if hot enough to produce such a result could hardly be pleasant to take and we have no evidence to show that numbers of people habitually take their food and drink at a temperature which is unpleasant to themselves; even if they did so, the irritation would last at most but a few minutes at a time at intervals of several hours, even supposing that all food at every meal were taken at a very high temperature; the commonest site of cancer of the stomach would not be reached until after the food had cooled. It is difficult to see how meat can act in such a manner as to produce inflammation similar in degree and nature to that produced by the various irritants which are accepted as causes of external cancer. Much the same may be said with regard to the other articles of diet mentioned. Diet may be among the causes of cancer but we have not sufficient evidence at present to say that it is. Trustworthy statistics are available only in the case of some of the most civilised countries and even then are insufficient and unsatisfactory in many respects. If it were possible to compare the death rate from cancer in populations which did and did not use alcohol, meat and other articles of diet, by means of equally trustworthy statistics, it would be reasonable to form a definite opinion upon these points; but reports of missionaries and medical officers serving abroad as to the frequency of cancer

¹ Rollo Russell, *Preventable Cancer* (Longmans, London, 1912).

cannot be used for purposes of comparison with statistics dealing with countries in which the whole population and causes of death are registered. As far as we know, there is no community of men existing under any kind of conditions in which cancer does not occur and those races in which cancer is said to be least common are generally those about which we know least. Cancer is apparently as common or nearly as common among mice as among men. Mice are the only animals which have been kept in vast numbers in laboratories under careful observation for the purpose of cancer research.

Recently it has been suggested by Lazarus-Barlow that there is a connexion between radium and cancer.¹ He says: "Radium appears to be found somewhat more frequently and in larger though still minute quantity in carcinomatous than in non-carcinomatous tissue; but the point is not yet certain, since in three instances in which carcinomatous and non-carcinomatous tissues were obtained from the same body and in which radium was found, it was present in larger quantity in the non-carcinomatous tissue."

A more suggestive set of figures are those given by this same observer² in connexion with the occurrence of gallstones in cancerous and non-cancerous cases. During the years 1900-4 inclusive, autopsies were made upon 1,448 individuals above the age of 35 years: of these 699 were cancerous, 749 non-malignant; among the 749 non-malignant cases, gallstones were found in 37, that is 4.94 per cent. The cases of cancer are divided into those suffering from primary cancer of the gall-bladder and those suffering from cancer in other parts of the body. Amongst the 693 cases of cancer elsewhere than in the gall-bladder, gallstones were found in 59, that is in 8.51 per cent.; but gallstones were found in all the 6 cases of primary cancer of the gall-bladder. The latter proportion may, however, be too high, as Colwell,³ dealing with a period of 50 years, states that gallstones were discovered in only 27 out of 31 cases of primary malignant disease of the gall-bladder and bile passages at the Middlesex Hospital, that is to say that gallstones were found in 87.1 per cent. of cases of primary cancer of the gall-bladder. Lazarus-Barlow gives the following figures as to the amount of radium, in the cases dealt with by

¹ *Arch. Middlesex Hosp.* 11th Report, Cancer Research Lab. 1912.

² *Op. cit.*

³ *Arch. Middlesex Hosp.* 4th Cancer Report, 1905.

him, estimated per gramme of gallstone by the ether extraction method and also by the incineration method :

Cases.	Frequency of gall-stones per cent.	Amount of radium per gramme of gall-stone.	
		Ether extraction method.	Incineration method.
Non-malignant	4.94	12.7×10^{-10} mgr.	0×10^{-10} mgr.
Carcinoma primary at sites other than gall-bladder	8.51	47.9 "	2.1 "
Primary carcinoma of gall-bladder	100 or 87.1	314.3 "	468 "

Granting the accuracy of the observations, there seems to be no doubt as to the correlation between cancer and gallstones, more particularly primary cancer of the gall-bladder. Also there does not appear to be any doubt that in the gallstones occurring in cases of cancer, again more particularly in cases of primary cancer of the gall-bladder, a larger quantity of radium was present in the malignant than in the non-malignant cases. But it is difficult at present to see what the real significance of this may be. Lazarus-Barlow suggests that radium is found more frequently and in larger quantities in cancerous than in non-cancerous tissues but does not show whether more radium is present in the tissues generally of a cancerous than of a non-cancerous subject. Is then the radium in the gallstones and the frequency of the occurrence of gallstones in cases of cancer secondary to the cancerous condition; or is the presence of radium the possible cause of the cancer? Neither supposition involves the belief that gallstones in themselves or the presence in them of radium are causes of cancer. Lazarus-Barlow claims that the nucleus of a gallstone may collect radium; it may be that if an excess of radium in the tissues of the organism be connected with cancer, this excess must exist for a long period and is accentuated in the gallstones. These, however, are speculations into which Lazarus-Barlow himself has not entered. We know that in many cases cancer follows upon prolonged irritation and that radium acts as an irritant but in the present state of knowledge it is hardly safe to form a definite opinion upon the matter.

EXPERIMENTAL WORK BEARING UPON A CURE

We may dismiss the various advertised cancer cures without any detailed comment: there is no evidence in favour of any

of them which will bear scientific investigation. What I have said with regard to diet as a possible cause of cancer applies even more forcibly to diet as a cure. Diet *may* be among the causes of cancer but when once a group of cells has become malignant, it is quite obvious that no change of diet can destroy them. The cancer cells derive their nourishment from the cells forming the body of the organism in which they exist. The cancer cells have been shown experimentally to possess a vitality at least as great as and in some respects greater than that of the somatic cells, so any change of diet must affect the cells through which the nourishment of the cancer cells passes before it affects the cancer cells. There is nothing that suggests that any particular form of diet could act upon the somatic cells in such a manner as would cause them to produce anything which would act in a selective manner upon the cancer cells and cause them to die out without affecting any of the other cells which form the body. Everything we know which bears upon this point suggests that such an effect could not be produced in such a manner. However, in spite of the extraordinary improbability that diet could affect the growth of cancer to any material extent, I made some experiments upon mice suffering from cancer in order to make sure of this point. Some were given a mixed diet of bread, water, milk and meat; others were kept upon a diet of rice and water only. According to certain claims that have been made from time to time, meat is one of the principal articles of diet which is to be avoided in cases of cancer. The tumours in all these mice grew at about the same rate. The great difference between the two sets was that the mice fed on a mixed diet thrived whilst those on a rice diet did not.

Radium and X-rays have been much used in cases of cancer and in some cases have been successful; there is, however, no evidence to show that these exert any specific action upon the cancer cells. The effect in both cases probably is to kill the cells which are exposed to the treatment, whether they be malignant cells or not. The form of malignant growth in which such treatment has been most successful is rodent ulcer. But rodent ulcer is successfully treated by purely mechanical means such as scraping, though more scarring is thus produced. There is therefore nothing in the effect produced in these cases which suggests selective action nor is there in

the cases of small superficial cancers which are cured in the same way. At the present time the only reasonable chance of producing a cure is that afforded by the total removal of all the cancerous cells. This can frequently be done successfully in superficial cancers which are recognised early; it is not commonly possible in cases of internal cancer, which are generally not recognised until the cancerous cells have multiplied and migrated to an extent which makes their total extirpation an extraordinarily difficult if not an impossible achievement. In connexion with reports of cures it must be remembered that very occasionally a case of cancer recovers without treatment and that a certain diagnosis is often impossible without a microscopic examination of a portion of the growth. Even when such an examination is possible the diagnosis is sometimes doubtful, as the chronic inflammatory condition seems to merge almost insensibly into the malignant. Reported cures, therefore, which are based entirely upon clinical evidence, are to be received with considerable doubt, if indeed they be received at all. Isolated recoveries following a certain line of treatment must be regarded in the same way: serious consideration can only be given if a number of recoveries follow regularly upon a given treatment.

A great deal of experimental work has been done with animals in the hope of discovering a means of dealing with cancer in the human subject; practically all of this work has been carried out with cancers artificially produced by inoculations similar to those described in the last article.

Certain kinds of resistance to the grafting of tumours usually transmissible in mice have been demonstrated by a great many observers. In 1889 Wehr¹ recorded the spontaneous cure of some of his transplanted tumours. Subsequently Gaylord and Clowes² reported recovery to have occurred from 20 per cent. of the Jensen mouse tumours; many others have reported similar occurrences. Gaylord and Clowes also found that the mice which recovered were immune to a further inoculation and that 10 out of 30 were resistant to a third and more virulent tumour. Ehrlich³ was successful in immunising mice against malignant tumours by inoculating with a non-malignant tumour. Others have

¹ *Arch. f. klin. Chir.* Berlin, 1889, xxxix. 225.

² *Johns Hopkins Hosp. Bull.* Baltimore, 1905.

³ *Arch. a. d. k. Inst. etc.* 1906, viii. 481.

failed to produce immunity in such a manner. Schöne,¹ Borrel and Bridré,² Bashford³ and Tyzzer⁴ have shown that inoculation with various normal tissue cells produces a variable immunity to the subsequent inoculation of usually transmissible tumours. Flexner and Jobling⁵ showed that from the tenth to the thirteenth day after the inoculation of heated tumour cells, the animals were more susceptible to inoculations with the living cells of the same tumour, suggesting by this experiment that a form of anaphylaxis was produced. Gaylord, Clowes and Baeslack⁶ injected mice suffering from tumours with the serum of immune mice. At first their results were highly satisfactory and many of the tumours disappeared, whilst normal serum produced no result. Subsequent experiments, however, were not satisfactory. Beebe and Crile,⁷ having drawn off a large proportion of the blood of some dogs bearing well-established transplanted sarcomata, transfused large quantities of blood from dogs that had resisted inoculation or recovered naturally; nine of the affected dogs recovered rapidly and completely. In 1908 I injected the serum of rats that had been subjected to repeated inoculations with the living cells of a rapidly growing mouse-carcinoma into mice bearing well-established tumours of the same strain; the result was that in 80 per cent. of the mice the tumours were completely absorbed. The serum of rats into which the living cells of the mouse's testis had been injected produced similar but less satisfactory results.⁸ Subsequent experiments with these sera showed that they were highly destructive to these particular tumour cells.⁹ These experiments were confirmed up to a point by Bashford,¹⁰ who showed that while the mouse-tumour cells lived in untreated rats for some time, they were rapidly destroyed in rats that had been previously inoculated with the living cells of mouse tumour.

Ehrlich¹¹ has explained the immunity to inoculation by means

¹ *München med. Wochenschr.*, 1907, liv. 2517.

² *Bull. de l'Inst. Pasteur*, 1907, v. 605.

³ *Scientific Rep. Imper. Can. Res. Fund*, 1907.

⁴ *Journ. Med. Res. Boston*, 1907, xvii. 155.

⁵ *Proc. Soc. Exper. Med. and Biol.* 1907, iv. 156.

⁶ *Med. News Philadel.* lxxxvi. 91, 1905.

⁷ *Proc. Soc. Exper. Med. and Biol.*, 1907, iv. 118.

⁸ *Lancet*, Sept. 12, 1908.

⁹ *Ibid.* April 9, 1910.

¹⁰ *Proc. Roy. Soc.*, B, vol. lxxxii. 1910.

¹¹ *Op. cit.* 1905; Apolant, *op. cit.* 1906.

of his hypothesis of "Atrepsia." In his opinion the immunity is connected with the nutrition available for the tumour cells. He found that when transferred for a short time to a rat and then back to a mouse, the tumour cells continued to multiply again with their original vigour; a continuance of this zigzag method of transplantation did not render the tumour less transmissible in the mouse, though it would die out if left too long in the rat. He assumed two kinds of atreptic immunity, both dependent upon what he calls "X-stoff," which supplies the tumour cells with nutriment, either directly or indirectly. In the one, in cases in which the tumour is in an animal similar to that in which it originated, the "X-stoff" facilitates absorption; in the other, in cases in which the tumour is transferred to an animal of another species, the part of the "X-stoff" which is itself carried over with the graft forms the nutriment of the tumour cells and is soon consumed. The "X-stoff," on this assumption, must obviously be produced continuously when the tumour cells, transferred to a similar animal, continue to grow indefinitely but is used up gradually in cases of immunity. I have kept a strain of tumour, given to me in 1906 by Prof. Ehrlich, growing in mice and have produced several hundreds of pounds weight of it, without any changes taking place excepting such as can be accounted for as the result of experimental treatment.

It seems quite in accord with other facts that mouse tumour should die out when inoculated into rats, as many normal mouse tissues have been shown to behave in the same way and the same thing happens if normal tissue of one kind of animal be introduced into another kind. The striking fact connected with this is, that the cells from one species of animal will sometimes multiply for a certain time in the bodies of another species before they are destroyed but this appears to happen only when the species are fairly nearly related. Jobling¹ has shown that transplanted pieces of a malignant growth from a human subject continued to grow in monkeys during a maximum period of sixteen days but failed entirely to grow in rats and mice. The new environment evidently supports the transferred cells during a time proportionate to its similarity to the natural environment. If the new environment be so nearly alike to the original that the most resistant cells survive, the action of selection may produce

¹ *Monographs of the Rockefeller Inst. for Medical Research*, No. 1, June 1910, p. 120.

a race of cells immune to it in the manner already indicated; on this supposition an "X-stoff" does not appear to be necessary to explain the various phenomena observed.

The experiments under consideration may be divided into two distinct groups: those in which the aim is to produce immunity to subsequent inoculations; and those which aim at curing already existing tumours. The results in both cases are evidently dependent upon the production in the body of the animal of a specific reaction against particular kinds of tissue. This reaction is shown best by the experiments demonstrating that after the introduction into the body of the animal of living cells which will be eliminated but slowly similar cells introduced soon after are eliminated much more rapidly.¹

The experiments showing the possibility of producing immunity to subsequent inoculation do not suggest a possibility of leading to anything that may be of practical value with regard to the prevention or cure of cancer in the human subject. Any preventive measures of this nature would have to be applied to every human being for some time before the cancerous age was reached and continued throughout life, as the immunity is apparently only temporary. There is also another difficulty which will be referred to later on.

The experiments in which already existing tumours have been caused to disappear are on a different footing and at first sight seem far more promising. Jobling² has shown that the cells of a malignant tumour from the human subject will live and multiply during nearly as long a period in the body of a monkey (*Macacus*) as do the cells of a mouse tumour in the rat, a result which favours the view that the monkey's serum might be rendered destructive in a selective manner to the cells of a malignant growth in man in just the same way that rat serum has been rendered destructive to the tumour cells of the mouse. But it must be remembered that the experiments referred to were performed upon transplanted tumours and it has been shown that these tumours differ in many respects from primary carcinomata.

Selection apparently has produced a race of cells in these transplanted tumours which possess many more of the characteristics of independent organisms than do primary cancers and thus the tissues of the host have been caused to react

¹ Walker, *op. cit.* 1908 and 1910; Bashford, *op. cit.* 1910.

² *Op. cit.* 1910.

against them in a way in which they do not react against the cells of a primary growth. It therefore seems probable that a constituent of the serum destructive to these tumour cells would be more easily produced and would be active to a greater extent and perhaps in a different way from a serum active toward the cells of a primary growth. Indeed, it seems likely that it may be impossible to produce a serum active towards the cells of a primary growth upon these principles. Moreover, it seems very probable that the destructive capacity would only be exhibited towards the particular race of tumour cells which had produced the reaction, in which case it would be practically impossible to apply the method to the human subject, a large quantity of serum being necessary and a sufficient reaction produced only after a number of inoculations of considerable quantities of living cells into the secondary host. The same criticisms apply to the results obtained in producing immunity to subsequent inoculation with the transmissible tumours.

The experiments in which the rats were inoculated with the cells of the mouse's testis, which afforded striking but less satisfactory results than those in which the serum produced by inoculating with the mouse tumour was used, avoid the suggested difficulty with regard to the serum being active against the cells of one particular tumour only. But this method is inapplicable to man on account of the impossibility of obtaining sufficient material from the human subject. Only living cells are effective. In addition there is the insuperable difficulty of obtaining the living cells of the human testis in sufficient quantities and often enough. The cells of the testis do not die immediately upon the death of the individual but practically all are dead in about three hours.

Many attempts have been made to find chemical compounds capable of exerting a selective action upon cancer cells—that is to say, which will kill the cancer cells without materially injuring the rest of the body. Wassermann¹ has recorded the effects produced by a preparation of selenium and eosin upon cancerous tumours produced by inoculation in mice. The preparation was introduced by intravenous injection directly into the circulation and after a number of injections produced a liquefaction of the tumours in the mice which survived the

¹ "Beiträge zum Problem: Geschwülste von der Blutbahn aus therapeutisch zu beeinflussen," *Deut. m. Woch.*, December 1911.

treatment. The treatment is stated not to have succeeded in the cases in which the tumour was larger than a cherry and the mortality produced by it appears to have been about 70 per cent. The theory of the treatment is based upon Ehrlich's statement that tumour cells possess a much greater avidity for oxygen and nourishment than do the cells of normal tissue.

Quite recently Neuberg, Caspari and Löhe have published the results of somewhat similar experiments.¹ These observers attribute the selective action of the preparations they have used to the presence in the tumour cells of certain enzymes which are absent from the cells of the body tissues. They bring forward in support of this view the rapid growth and the rapid degeneration of the tumour cells. Rapid growth is a characteristic feature of some strains of experimentally produced tumours in mice and rats and as shown in the last number of SCIENCE PROGRESS may probably be produced in all by a process of selection. Degeneration and death of the cells in the centre of these tumours is probably a characteristic of all strains but it is not of all kinds of malignant growths in the human subject, though it is perhaps more common in some kinds than is usually recognised. With regard to this point Ewing says² that he does not consider it has yet been proved that well-developed tumour tissue undergoes autolysis more rapidly than an equivalent normal tissue. As evidence to the contrary he quotes the experiments by himself and Beebe³ in which dog's blood was passed by artificial circulation through test tubes containing fragments of sarcoma from a dog. The fragments remained alive during from eight to ten days; fragments of dog's liver and kidneys became necrotic and autolysed in forty-eight hours under the same conditions.

Perhaps the most suggestive evidence with regard to the existence of a specific ferment in tumour cells is provided by the work of Beebe.⁴ From the purified nucleoproteids of cancer, he prepared a serum which agglutinated the emulsified cells of cancer and precipitated the nucleoproteids derived from this source but acted very feebly and only when used in large pro-

¹ "Weiteres über Heilversuch an Geschwulstkranken Tieren mittels tumoraffiner Substanzen," *Berl. klin. Woch.* July 22, 1912.

² "Cancer Problems," *Arch. of Internal Medicine*, vol. i. 1908.

³ Beebe and Ewing, *Brit. Med. Journ.* 1906, ii. 1559.

⁴ Quoted by Ewing in "Cancer Problems," *op. cit.*

portion on cells and nucleoproteids from normal tissue. These experiments however, as far as I know, have not been repeated.

Neuberg and his collaborators apparently assume that the peculiar characters of rapid growth and degeneration which they attribute to the cells of malignant growths are due to the presence in them of these abnormal enzymes; their object has been to produce some substance which will act only or to a greater extent in the presence of the enzymes in question and increase the degeneration to such an extent that all the tumour cells will be destroyed. They have worked with compounds of cobalt, silver, copper, platinum, gold and tin, obtaining the best results with compounds of the first two. While claiming that definite effects were produced upon the tumour cells by hypodermic injections, they say that they did not effect actual cures until they used intravenous injections.

In the case of these experiments, as in Wassermann's, the tissues are described as undergoing degeneration, softening, liquefaction and final disappearance. The useful dose of the compounds is nearly as great as that which kills the animal outright and must be injected into the circulation directly. This latter point adds to the difficulty of the experiments, as it is exceedingly difficult to inject a fluid into a vein in a mouse; moreover, as the operation has to be repeated frequently and the difficulty is increased rather than diminished upon each occasion, the experiment in each individual case may have to be abandoned before completion. It is also to be regretted that none of these investigators has given any definite information either as to the constitution or as to the manner of preparing the "compounds" they used, so that their experiments cannot be confirmed nor is any kind of check upon them possible; nor can the work be carried on by other investigators along varying lines from the new standpoint, as it probably would be if the results they have recorded were confirmed.

The immediate effect of the "compounds" injected by Neuberg and his collaborators is described as a contraction of the blood-vessels of the body and a dilatation of those of the tumour. This dilatation is so great that extravasations of blood visible to the naked eye are numerous. In Wassermann's experiments the injection of the selenium-eosin preparation was described as turning the mouse pink all over immediately but the pink coloration disappeared rapidly from the body and

was concentrated in the tumour. These facts seem to suggest that the action of the preparations under consideration may possibly be to some extent mechanical and not due to any selective action upon the tumour cells. We have seen that there is no nerve supply to malignant growths. The dilatation and contraction of blood-vessels is controlled by the nerves and hence it is possible that when these poisonous substances are introduced into the circulation the immediate result is the contraction of the blood-vessels generally, excepting of course those in the tumours, through their action upon the nervous system. The blood-vessels and spaces in the tumour, owing to the increased pressure produced by the contraction of the vessels of the body, are forcibly dilated. The poisonous compounds having been introduced directly into the blood stream would thus act far more upon the tumour cells than upon those in the body generally and as they are described as being very unstable they would break down before the blood-vessels of the body dilated. The fact that the doses that are effective in producing the destruction of the tumour are so very nearly those that result in the death of the animal is very suggestive in view of this explanation. So is also the fact that though it is reported that in the very few cases of spontaneous tumours in animals upon which these preparations have been tried, effusions of blood and softening have occurred more often than not, no cures have been obtained. The animals have always died before the tumour was destroyed.

As was explained in a previous article, the tumours produced in mice and rats by grafting become surrounded by a capsule of inflammatory tissue before cell proliferation begins among the tumour cells, so that these tumours are cut off from the body of the animal in which they grow in a manner not found to happen in a spontaneous primary cancer. This would very probably, to a certain extent, confine the poisonous compound to the tumour after it had been concentrated there through the contraction of the blood-vessels of the body generally and the concomitant dilatation of the blood-vessels and blood spaces of the tumour. That the blood-vessels of spontaneous tumours should become dilated in the same way is what might be expected but in such cases there is lacking that isolation of the tumour cells which forms so useful a factor in success when the curative and lethal doses are very

nearly balanced, as they are in the experiments under review. What was said in the last article with regard to these graftable mouse cancers having acquired some of the characters of separate individuals not possessed by primary malignant growths through the process of selection necessarily involved in their propagation must also be borne in mind.

Neuberg and his collaborators attribute the failure of subcutaneous injections to the other tissues having broken down the unstable compounds they used before the tumour cells were reached. The explanation with regard to the contraction of the blood-vessels that I have just suggested seems to be as satisfactory upon this point, as whilst contraction of the vessels would probably be produced, though more slowly, by the subcutaneous injections, the fluid would not be in the actual blood stream from the moment of its introduction into the system and so would not have a chance of being concentrated immediately in the tumour.

Wassermann describes amorphous particles of selenium and Neuberg and his collaborators amorphous particles of the metals which were used as being discernible under the microscope in the tumour cells. As the "compounds" they used are described as very unstable they would probably break down in any part of the body but being in greater quantity in the tumour when first introduced more breaking down should take place there than anywhere else.

A definite claim has been made recently to the successful treatment of cancer by intravenous injections of colloidal selenium.¹ As far as I know, only one or two cases have been treated in this manner, so that even a disappearance of the tumours would mean no more than that there was no direct evidence against the disappearance of the tumours being connected with the injection of the selenium in the colloidal form. I have tried colloidal selenium upon a number of mice and rats bearing malignant tumours produced by grafting. No effect was produced upon the tumours whether the colloid was used alone or in conjunction with eosin. It is noteworthy that whilst all the salts of selenium and combinations of selenium and eosin which I have tried are very highly toxic, minute doses killing mice or rats in a few minutes, selenium in the colloidal form is not at all poisonous.

¹ *Société Médical des Hôpitaux de Paris*, February 14 and March 1, 1912.

CONCLUSIONS

Having reviewed much of what has been done in the way of attempts to cure cancer, the only conclusion to be arrived at is that at the present time the only means available which affords any reasonable chance for the patient is complete removal by a surgical operation. Complete removal is generally only possible in the very early stages and the only cases, as a rule, in which there is a really good prospect of success are superficial cancers which are diagnosed very early. In many cases, however, much more may be done in the way of alleviation and the prolongation of life under more comfortable conditions than was formerly possible by surgical operations.

On the other hand, it should be thoroughly realised that we have learned much concerning the nature of cancer during the past ten years. Whilst none of the present lines of inquiry seem to promise immediate success, the results already obtained in following several of them serve to suggest the ultimate discovery of one or more methods of curing a large number at least, if not a great proportion, of cases of malignant disease.

It has been satisfactorily established that the only way in which cancer can be transferred from individual to individual is by the grafting of the living cancer cells in a suitable position. Even when this is done, it is successful only in the case of some particular tumours, as apparently all are not transmissible; and of graftings with usually transmissible tumours only a certain proportion are successful.

It may be said therefore, with certainty, that cancer is neither infectious nor contagious in the ordinary sense of these words and that there is no risk of catching cancer from a cancer patient unless in the highly improbable event of living cancer cells being introduced into an accidental wound incurred by the surgeon or his assistants during an operation.

THE DEATH-RATE OF EARTHQUAKES

By CHARLES DAVISON, Sc.D., F.G.S.

THE destruction of Messina at the close of 1908 has made us familiar with the immense loss of life that may be accomplished within a few seconds by a great earthquake. The total number of deaths is still unknown ; probably it will never be revealed but it cannot fall far short of 100,000. Seldom has this number been exceeded, though it has often been approached in other lands as well as in Italy. Taking the latter country first, we may recall the long series of earthquakes in 1783, when more than 30,000 lives were lost ; and the Sicilian earthquake of 1693, when the number rose to more than 58,000 according to Dr. Baratta and to 93,000 according to Prof. Mercalli. Smaller but still considerable figures were attained in other earthquakes, for instance, 2,313 in the Ischian earthquake of 1883, 6,240 in the Norcian earthquake of 1703, 12,291 in the Neapolitan earthquake of 1857 and 15,000 in the Sicilian earthquake of 1169.

The Japanese records tell the same tale. In 1891, 7,273 lives were lost during the great earthquake in the provinces of Mino and Owari. Five years later, 27,000 persons were drowned at Kamaishi and along the neighbouring coast by the sea-wave following an earthquake. To the Japanese, this wave was more costly in life than the whole war with China in 1894. Again, 30,000 persons were killed by the Kamakura earthquake of 1293 and the same number in Yechigo in 1828. But even these figures were surpassed in 1703, when the death-roll is said to have risen to 200,000, half of this number being in the district of Awa alone. In other countries, to give only a few more instances, we find that 50,000 were destroyed by the Lisbon earthquake of 1755, 40,000 in northern Persia in the same year, 60,000 in Cilicia in 1268, 100,000 in Pekin in 1731, 180,000 in India in 893, more than 80 per cent. of this number having been buried in the ruins of one city, whilst 300,000 are said to have perished in the Indian earthquake of 1737. "As yet," wrote Humboldt in 1844, "there is no manifestation of force known to us, including even the murderous

inventions of our own race, by which a greater number of people have been killed in the short space of a few minutes."

On the other hand, in some great earthquakes the loss of life has been surprisingly small. At Charleston in 1886, only twenty-seven were killed, though fifty-six more died afterwards from cold and exposure. At San Francisco, twenty years later, the earthquake was directly responsible for no more than 390 deaths; and the total number of lives lost at Kingston in 1907 is estimated at about 1,000.

In considering such statistics it is evident that the figures furnish no real test of the destructive violence of an earthquake. Some of the greatest shocks for many years past are those which have occurred in the sparsely inhabited regions of central Asia. The disastrous character of the Messina earthquake was chiefly due to the presence of a large and ill-built town near to its origin. The heavy death-rolls of earthquakes in India and China are to be attributed to the dense population of those countries. Consequently, instead of the death-roll, a more accurate measure would be the death-rate or the proportion deaths bear to the whole population. For instance, in Charleston during the earthquake of 1886 and more recently in San Francisco, the death-rate was considerably less than 1 per cent. In the Ischian earthquake of 1881, it amounted to $2\frac{1}{4}$ per cent. at Casamicciola. In the Andalusian earthquake of 1884, the highest death-rate at any place was 9 per cent. and in the Riviera earthquake of 1887 not more than 14 per cent. Though attracting great attention from their occurrence in well-known districts, these earthquakes belong to a group characterised by a comparatively small loss of life.

In contrast with the above figures, many of the Italian earthquakes are characterised by an unusually high death-rate. In the Ischian earthquake of 1883, the death-rate at Casamicciola was 41 per cent.; in the Sicilian earthquake of 1693, it rose to 50 per cent. at Ragusa and to 67 per cent. at Catania; in the Neapolitan earthquake of 1857, it was 50 per cent. at Saponara and 71 per cent. at Montemurro; in the first great Calabrian earthquake of 1783, 59 per cent. at Bagnara and 77 per cent. at Terranova; whilst in the Norcian earthquake of 1703, the highest death-rate at any place was 81 per cent. at Avendita. The corresponding figures for the Messina earthquake are not yet accurately known; at Canitello the death-rate was 44

per cent. but in the lower part of Messina itself and Reggio di Calabria the rates may well exceed any of those given above.

Among the conditions which determine whether the death-rate due to an earthquake shall be high or low may be mentioned the time of occurrence, the suddenness with which the shock begins and the rapid succession of strong after-shocks. These are all properties of the earthquake and beyond our control. There are also others of no less consequence, which are governed more or less by our own actions, such as the proximity of towns to well-known seismic centres, the nature of the site selected—whether on sloping or level ground, on a rocky or loose foundation—and the nature of the buildings. I propose to consider these conditions in detail, as it is only from a knowledge of such conditions that we can expect to discover means of mitigating, when we cannot altogether prevent, the disastrous effects of great earthquakes.

The time of occurrence is one of the most important factors. An earthquake which occurs at night is nearly always more disastrous than one in the daytime. Not only are people gathered indoors but, if asleep, they are unable to take advantage of the brief warning that is sometimes given by the preliminary sound or tremor. Among earthquakes with a high death-rate may be mentioned the Messina earthquake of 1908, which occurred at about 5.20 a.m., the Ischian earthquake of 1883 at 9.25 p.m., the Neapolitan earthquake of 1857 at 10.15 p.m., the Kangra and Dharmsala earthquake of 1905 shortly after 6 a.m. and the great Indian earthquake of 1737 at night. Among those with a low death-rate are the Assam earthquake of 1897, which occurred at 5.15 p.m., the Ischian earthquake of 1881 at 1.5 p.m., the Kingston earthquake of 1907 at 3.30 p.m., the Port Royal earthquake of 1692 and the first Calabrian earthquake of 1783 which happened shortly before and after noon. But even the daytime loses its advantage when, owing to religious celebrations, many people are congregated within doors. The Riviera earthquake of 1887, for instance, took place on an Ash Wednesday morning at twenty minutes past six. After a night spent in amusement, many persons had lain down and were sleeping heavily; others had risen early and were gathered together in churches. The Caraccas earthquake of 1812 occurred at 4.7 p.m. on Ascension Day. "The procession of the day," says Humboldt, "had not

yet begun to pass through the streets but the crowd was so great within the churches that nearly three or four thousand persons were crushed by the falling of the roofs."

The suddenness of onset of the shock is a second factor of considerable importance. Almost invariably the shock is preceded by a deep rumbling sound accompanied by a faint tremor which may last five or more seconds before the vibrations attain a destructive strength; the same sound precedes both weak and strong shocks and at first affords no certain warning of the disaster but in earthquake countries it is one that is always heeded. "If it had happened in the middle of the night," wrote Darwin of the Concepcion earthquake of 1835, "the greater number of the inhabitants . . . must have perished, instead of less than a hundred; as it was, the invariable practice of running out of doors at the first trembling of the ground alone saved them. In Concepcion each house or row of houses stood by itself, a heap or line of ruins." To the same cause may be attributed the comparatively small loss of life in such earthquakes as those which destroyed Cumana in 1797 and Port Royal in 1692.

In many earthquakes, however, the warning given by the earthquake sound is too brief to be of service. This was the case, even with those who were awake, at Dharmasala in 1905 and at Messina in 1908. In the Ischian earthquake of 1883 sound and preliminary tremor were both absent within the central district. So suddenly and with such intense violence did the shock begin that survivors at Casamicciola found themselves beneath the ruins of their houses before they realised that an earthquake had occurred.

The death-rate of an earthquake is often increased by the rapid succession of strong after-shocks. In the central district every great earthquake is followed by almost incessant tremors among which stronger shocks are interspersed. The Riviera earthquake of 1887 occurred at about 6.20 a.m. At 6.29 there followed a second shock and at 8.51 a third of intermediate strength. To these two shocks are attributed one-quarter of the total amount of damage and also the small number of wounded, many of those who lay buried in the ruins having been killed by the subsequent overthrow of the shattered walls. In this earthquake the number of persons wounded was only 72 per cent. of the number killed. The Neapolitan earthquake of 1857 was succeeded after about an hour by another strong

shock and the number of wounded was only 14 per cent. of the number killed. In the Andalusian earthquake of 1884 and the Japanese earthquake of 1891, on the contrary, the number of wounded was more than double that of the number killed.

Of the remaining conditions, the harmful effects of which we can to a certain extent restrain, the most important is the proximity of towns to well-known seismic centres. The unstable regions of the earth have been determined on a large scale by M. de Montessus de Ballore and Prof. J. Milne, the map constructed by the former being based on all recorded shocks and that of the latter on world-shaking earthquakes. The dangerous zones of certain countries, such as Italy and Japan, have also been carefully delineated. In Europe the large towns are far removed as a rule from earthquake centres. Those which have suffered most are Lisbon, Catania and Messina, in addition to a number of small towns in the south-east of Spain, in Ischia, Calabria, the Balkan peninsula, the Ionian Islands, Crete and several islands in the Grecian archipelago. Other countries are less fortunate. Off the west coast of South America and especially from the tenth to the fortieth degree of south latitude, the steeply shelving ocean-bed marks the site of one of the most unstable portions of the globe. Nearly all the larger towns on the coast—Callao, Lima, Arequipa, Iquique, Copiapo, Coquimbo, Valparaíso, Concepcion and Valdivia—have been destroyed at some time or other, most of them more than once, several having suffered from the rush of the great sea-waves as well as from the force of the shock. The shores of the Pacific Ocean, indeed, are specially subject to seismic disturbances throughout a great part of their extent. Of the 675 "world-shaking" earthquakes which have been studied by Prof. Milne during the eleven years 1899–1909, three-fifths have originated in the five zones which border that ocean, the greater number being submarine. Five other zones are entirely oceanic but these and a sixth zone containing the West Indian Islands include only one-fifth of the total number of earthquakes, the remaining fifth originating in a great terrestrial zone extending from Italy eastwards to the Himalayas.

The most important feature of these seismic zones from our present point of view is that earthquakes shake particular portions time after time, although they occur in other places in the intervals. As on the west coast of South America, the

earthquake of 1887, it is estimated that more than 90 per cent. of the dead bodies were found crushed beneath fallen arches.

Of the six conditions which govern the high death-rate of earthquakes, we are chiefly concerned only with the last three. We cannot in any way limit the time of occurrence of a great earthquake nor can we prevent the rapid succession of strong after-shocks. Fore-shocks when they occur and the preliminary sound may provide early notice of the coming shock but, unfortunately, they are characteristic of slight as well as of disastrous earthquakes. Weak shocks may come alone and we cannot distinguish between such isolated tremors and the forerunners of a catastrophe. Moreover, when they assume the latter aspect, the interval that may elapse before the great shock comes is of uncertain duration. It may be a few minutes or hours, it may amount to days or weeks. The preliminary sound and tremor differ in this respect. Both precede the shock by a few seconds; and except in large and lofty buildings a warning of even five seconds may be sufficient. Pheasants and other birds are often terrified by the early tremors of an earthquake; but when kept by the late Prof. Sekiya for the purpose they failed to serve as satisfactory heralds. The deep earthquake-sound, again, is not equally audible to all persons. It is so low that to some, who are not in the least deaf to ordinary sounds, it is quite inaudible. There is also reason to believe that races differ in their capacity for hearing the earthquake-sound; and it is possible that a general deafness towards the earthquake-sound may result in raising the death-rate. When this defect exists, it might perhaps be remedied by the use of sensitive flames adjusted so as to respond to the deepest sounds alone.

All attempts to issue earthquake-warnings have failed and have deserved to fail, for the supposed forecasts have been based on insufficient data. Without some knowledge of the origin of earthquakes and of the movements which precede the final catastrophe, such attempts were of necessity futile. But, with the recent growth of our knowledge, it seems by no means impossible that we may in time be able to provide rough forecasts of a coming shock. To be of service, such forecasts should give the approximate time at which an earthquake may be expected and the region in which its severity will be chiefly concentrated. To furnish both elements is at present beyond our powers. But to give one only may be useful and of the

two elements it is of greater value to know the area that will be mainly affected than the time when a shock will take place. The time alone would be of little service, for sixty "world-shaking" earthquakes occur on an average every year, so that as a rule few weeks will pass by without the visit of an earthquake somewhere or other upon the globe.

What is required for the solution of this problem is more definite knowledge than we at present possess of the operations which precede the occurrence of a great earthquake. On this subject, some light has been thrown by recent disasters. A displacement of the earth's crust along a fracture more than two hundred miles in length, like that which caused the Californian earthquake of 1906, cannot be the work of an instant of time. For many years, the strain must have been increasing until it reached the point when rupture and sliding could no longer be averted. By the erection of pillars along a line at right angles to such a fracture and by careful observation subsequently of their relative positions, the first deformations may be detected and measured. Or, again, before a great movement can take place, small obstacles to motion must be cleared away along the surface of the fracture and every such removal must give rise to a tremor more or less pronounced. The outlining of the course of a fault by the centres of numerous slight shocks, as happened before the Japanese earthquake of 1891, should reveal the preparation that is being made for a great movement—a movement which may, as in that case, take place within the next two years.

For the present, it would seem advisable to direct attention to those conditions which are partially within our control, so as to lessen, if we cannot avert, the destructiveness of an earthquake shock. In a few cases, there can be little doubt that the Government should interfere and prohibit the rebuilding of a town that has been frequently ruined. In permitting the re-erection of Casamicciola after the Ischian earthquake of 1883, the Italian Government incurred a grave responsibility, notwithstanding all the precautions taken. Here, there is no reason to suspect any migration of the seismic focus. Time after time, the same small district has been the seat of renewed shocks of increasing violence. The central volcano of Epomeo may have been extinct during the historical period but outbursts have occurred along radial fissures. The violent shocks which

preceded the last eruption in 1302 were similar to those which have occurred recently in the island and there is reason to fear that the Ischian earthquakes of 1796, 1828, 1881 and 1883 are merely symptoms of underground activity which sooner or later may result in forming a new lateral cone on the present site of Casamicciola.

Of most towns, especially of those which lie along the coast, the partial removal is all that can be considered. A harbour like that of San Francisco, which has no rival for hundreds of miles and which lies close to the shortest route from Panama to Yokohama and Shanghai, cannot be transferred. Nor can those along the western coast of South America, subject though they be to the inrush of seismic sea-waves. The utmost that can be attempted in such cases is to shift the residential quarters farther inland, just as, after the earthquake of 1692, Port Royal was maintained as a naval station while the town of Kingston arose in place of that which sank beneath the sea. The removal of a town, however, is a remedy so desperate that it will seldom be entertained; and as the recent experience of Kingston has shown it may not be altogether effectual. We must, therefore, as a rule, avail ourselves of the alternatives at our disposal and endeavour to mitigate the effects of earthquakes by the choice of suitable sites and modes of building.

As regards situation, it is clear that, in the absence of a protecting sea-wall, low-lying land along shores that are liable to be swept by seismic sea-waves should be avoided. All buildings, especially lofty ones, should be erected on a rocky foundation, never if otherwise possible on sand or gravel. Soft friable beds resting on a slope of rock or forming the edge of a cliff or steep river-bank are perhaps the worst of all foundations. Not only is the shock more strongly felt on them than on the adjoining rock but the beds as a whole may slide downwards or forwards and be extensively fissured by the action of the shock.

In all cases, however, even in those in which an inferior site cannot be avoided, the loss of life may be diminished by erecting only houses that are adapted to withstand the strain of an earthquake shock. To erect a building that is comparatively earthquake-proof and at the same time fire-proof is merely a question of expense. The walls must be very strong at the base and as light towards the top as may be consistent with strength; they must be firmly braced together by iron rods from front to

back, from end to end and from foundation to roof, so that the whole may vibrate practically as one mass. Public buildings should be of this type; but in the case of ordinary dwelling-houses the expense of such methods would be prohibitive. Fortunately, approximate safety in such cases may be secured by other and less costly means. During his long and fruitful residence in Japan, Prof. Milne determined the principal conditions which should govern construction and the following description of an ideal house is founded on the conclusions at which he then arrived.

The houses are built in wide streets, with deep foundations and are not as a rule more than two storeys high. The walls are at once light and strong. They consist of a framework of wooden beams, firmly braced together, the intervening spaces being filled with light stone or hollow bricks. There are no gable-ends and the corners of the houses are specially strengthened. Nor are there any arches, except perhaps in the cellars and then they are high, curve into the abutments and are protected above by a lintel of wood or iron. The openings for doors and windows in successive storeys are not placed in a vertical line and are at some distance from the corners of the house. The roofs are light and low-pitched and all tiles, if used, are fixed by nails. The floor-beams in alternate storeys are at right angles and penetrate nearly the whole thickness of the walls. Chimneys, if forming part of the house, are short and thick and without heavy ornamental copings; if in the centre, they penetrate the roof without touching it. Balconies are altogether absent and the staircases, if connected with the main walls, are light. No portions of the house are allowed to vibrate separately from the rest and with different periods. The one object throughout is to produce a light, strong and fairly elastic house, which, in the day of trial, shall vibrate as a whole and, while bending before the shock, shall yet endure.

How greatly such methods may contribute to the saving of life has been admirably illustrated by Prof. Omori in his recent report on the Messina earthquake. On October 28, 1891, a violent earthquake devastated the provinces of Mino and Owari in Japan. The shock was more than four times as strong as the Messina earthquake and was felt over an area ten times as great but the total number of victims was only 7,273. Not far from the origin of the earthquake lies the city of Nagoya with a

population in 1891 of 165,000. Here, though the intensity of the shock was slightly greater than at Messina, only 190 persons lost their lives instead of about 75,000 at the latter city. Thus, taking the difference of population into account, the number of persons killed in Messina was about 430 times as great as in Nagoya; or as Prof. Omori forcibly remarks, about 998 out of every thousand persons killed in Messina fell victims to the faulty construction of their houses.

THE CHEMICAL ACTION OF LIGHT ON ORGANIC COMPOUNDS

By W. A. DAVIS, B.Sc.

WITHOUT question the most important chemical change induced by light is the transformation of carbon dioxide into sugars and starch under the influence of the chlorophyll of green leaves, involving as it does the absorption of much energy. In most cases studied, light brings about a change involving a loss of energy; in the minor number of cases in which energy is undoubtedly absorbed and its amount can be approximately calculated, the absorption, expressed in thermal units, is exceedingly small: thus in the polymerisation of anthracene to dianthracene, which has been studied by Luther and Weigert,¹ the amount of energy absorbed, though greater than in most other cases, is yet only about forty calories per gramme of anthracene transformed. In the formation in the leaf of each gramme of starch from carbon dioxide and water, an amount of energy represented by 4,230 calories must be supplied; this is more than 100 times as great as that absorbed in any other known photochemical change. The rapidity also of the synthetic action effected in plant foliage is far greater than that observed in the majority of other photochemical changes, especially in comparison with those in which energy is absorbed. The assimilation of carbon in the plant is, in fact, an unique phenomenon. The object aimed at in the present article is to give an account of the experimental work which has been carried out during the past few years, especially by Professors Ciamician and Silber at Bologna, to obtain direct information as to the general character of the changes brought about in organic compounds by the action of light.²

¹ *Zeit. Phys. Chem.* 1905, 53, 416.

² Three monographs on the chemical action of light have been published recently: (1) *Die Chemische Wirkungen des Lichts*, by Fritz Weigert (Ahrens' *Sammlung Chemischer und Chemisch-technischer Vorträge*, 1911, vol. 17, pp. 183-296); this deals with the question mainly from the physical side. (2) *Les Actions chimiques de la Lumière*, an address delivered by Prof. Ciamician before the Chem. Society of Paris (*Bull. Soc. Chim.*, 1908), in which his experiments up to that date are discussed. (3) *Photochemie*, by Joh. Plotnikow (Knapp, Halle-Sa., 1910), a general treatise.

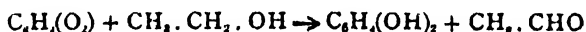
The changes effected by light in carbon compounds may be classified as follows:

- (1) Oxidation and reduction (reciprocal),
- (2) Autoxidation,
- (3) Polymerisation,
- (4) Condensation and synthesis,
- (5) Isomeric and stereoisomeric change,
- (6) Ring scission and hydrolysis,
- (7) Two or more of these changes simultaneously.

All the transformations dealt with in this article occur only under the influence of light: that this is true has been ascertained in every case by a control experiment in which the materials that were found to interact in light were left in darkness during a period equal to that of the exposure to the sun's rays without producing any positive result.

I. OXIDATION AND REDUCTION

The largest proportion of the changes studied are in this class; reciprocal oxidation and reduction of two substances, one of which is oxidised at the expense of the other, appears, in fact, to be the type of photochemical action most easily brought about. In many cases the change effected consists simply in the transference of one or more hydrogen atoms from the one compound to the other. Thus in the first case studied by Ciamician and Silber, in 1886,¹ when a solution of quinone in aqueous alcohol was exposed to light in sealed glass vessels the yellow-coloured quinone disappeared, giving place to colourless quinol, an equivalent of aldehyde being produced at the same time; some quinhydrone was also formed by the interaction of quinol and quinone.



This work was not carried further at the time but in 1901² it was found that under similar conditions quinone was capable of oxidising isopropyl alcohol to acetone, being itself, as before, reduced to quinol. Tertiary butylic alcohol (trimethylcarbinol) is also oxidised by quinone, quinol and quinhydrone being formed as secondary products but the nature of the substances into which the alcohol is converted is uncertain; the action

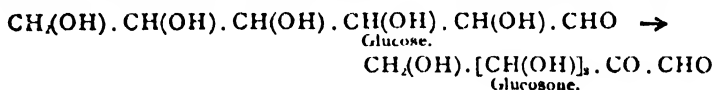
¹ *Gazzetta Chimica Italiana*, 1886, **16**, 111.

² *Atti R. Accad. Lincei*, 1901, **10**, i. 92.

takes place much more slowly than in the case of either of the other alcohols.

Perhaps the most interesting of the oxidations effected by quinone under the influence of light is that of the polyhydric alcohols, such as glycerol, erythrol, *d*-mannitol and dulcitol, each of which loses two atoms of hydrogen and is converted into the corresponding aldehyde.

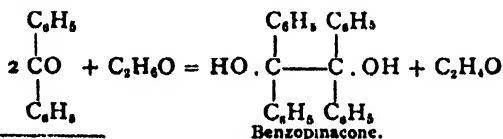
These changes are striking in so far as previously they were known to occur only under the influence of relatively powerful oxidising agents, such as nitric acid or an alkaline hypobromite. A particularly interesting case is the oxidation of glucose by quinone in sunlight to glucosone, as follows:



Other quinones behave with alcohols in the same way as benzoquinone; this is especially true of thymoquinone, which in ordinary alcoholic solution gives thymoquinol and aldehyde. The action of phenanthraquinone or isatin on alcohol takes place, however, very slowly.

Formic acid is fairly rapidly oxidised by quinone in sunlight to carbon dioxide, quinol being the other product.¹ The fatty acids (acetic and propionic acid) are only very slowly affected and the nature of the products (other than quinhydrone) could not be ascertained. Hydroxy-acids (lactic, malic and tartaric) are oxidised by quinone to carbon dioxide; the corresponding keto-acid could not be isolated.

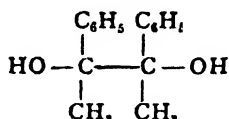
But quinones are not the only compounds which are capable of being reduced by alcohols under the influence of sunlight: ketones and aldehydes are similarly affected, albeit, as a rule, much more slowly and incompletely. In the case of the paraffinoid ketones, the action is of a complex character (see Section VII.) but is relatively simple in the case of benzenoid ketones: thus, benzophenone (4 grm.), in presence of alcohol (20 cc.), is transformed in eight days almost completely into benzopinacone:²



¹ *Atti R. Accad. Lincei*, 1901, 10, i. 92.

² *Ber.* 1900, 33, 2911.

Acetophenone, in a similar manner, gives acetopinacone :



In these cases, the simple reduction to secondary alcohol is masked by the tendency of two molecules of the latter to undergo oxidation to form a pinacone. Benzopinacone is also largely formed when benzophenone is exposed to sunlight in certain hydrocarbon solvents, such as toluene, ethylbenzene and the xylenes but other changes also occur in such cases (see Section VII.).¹

Benzoin in alcoholic solution is reduced to hydrobenzoin (its stereo-isomeride *isohydrobenzoin* being formed at the same time) but a quantity of resin is also produced.² The main action, however, is that expressed by the equation :

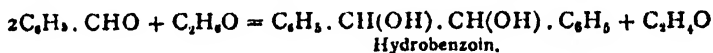


When the diketone benzil, $\text{C}_6\text{H}_5 \cdot \text{CO} \cdot \text{CO} \cdot \text{C}_6\text{H}_5$ is exposed to light³ in alcoholic or ethereal solution during a few hours, it is partly reduced to benzoin, which combines with unchanged benzil to form *benzilbenzoin*,



a loose molecular compound which is resolved into its components when melted or when boiled with benzene or alcohol. If the action of light be prolonged, the benzilbenzoin which has separated redissolves and a quantity of resin is formed together with benzoin, benzil, benzoic acid and ethylic benzoate.⁴

The action of light on benzaldehyde is particularly striking on account of the variety of products. The simplest product,⁵ obtained in alcoholic solution, is a mixture of the two stereoisomeric hydrobenzoin formed by reduction, thus :



A complex, polymerised form, $(\text{C}_{11}\text{H}_{11}\text{O}_2)_n$, of hydrobenzoin is also produced. The polymerisation of benzaldehyde alone under

¹ *Atti R. Accad. Lincei*, 1910, 19, i. 645.

² *Ibid.* 1901, 10, i. 92.

³ Klinger, *Ber.* 1886, 19, 1862.

⁴ Ciamician and Silber, *Atti Lincei*, 1903, 12, i. 235 ; *Ber.* 1903, 36, 1575.

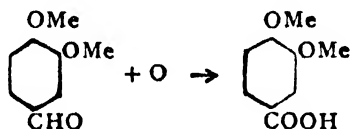
⁵ *Atti R. Accad. Lincei*, 1901, 10, i. 92.

the influence of light is dealt with later (p. 266); the remarkable self-reduction which takes place in presence of traces of iodine is considered on p. 261.

Anisaldehyde behaves in alcoholic solution in the same manner as benzaldehyde, giving rise to hydranisoin but the action is much slower. The behaviour of vanillin,¹ on the other hand, is quite special in its character, oxidation occurring when a solution of the aldehyde in alcohol, ether or acetone is exposed to light; the product is *dehydrovanillin*, which can also be obtained by the oxidation of vanillin with weak oxidising agents, thus:



Puxeddu² has recently shown that the photochemical oxidation of vanillin does not depend on the presence of atmospheric oxygen but takes place equally well in sealed tubes when it is dissolved in benzene or toluene; it is probably a case of self-reduction in which vanillyl alcohol is also formed. The action is always particularly rapid, dehydrovanillin beginning to separate from the solution after ten minutes' insolation. It is interesting that the ethylic and methylic ethers of vanillin behave in an entirely different way. Thus when a solution of methylvanillin or of ethylvanillin in alcohol, benzene, toluene or acetic acid is exposed in sunlight, the corresponding vanillic acid is formed:



Puxeddu gives a rather complex explanation of these changes which involves the assumption that a dibenzylidene derivative [in the case of the methylic ether, this has the structure $(\text{OMe})_2 \cdot \text{C}_6\text{H}_3 \cdot \text{CH} : \text{CH} \cdot \text{C}_6\text{H}_3(\text{OMe})_2$] is formed as intermediate product; but no trace of such a compound could be isolated. It seems more probable that what really occurs is self-oxidation involving the formation of the vanillic acid and the corresponding vanillyl alcohol such as actually has been shown by Mascarelli to take place in the case of benzaldehyde in presence of traces of iodine (see p. 261).

When benzaldehyde dissolved in benzylic alcohol is exposed

¹ *Atti R. Accad. Lincei*, 1901, 10, i. 92.

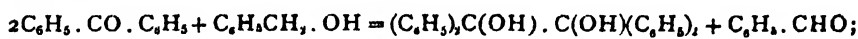
² *Ibid.*, 1911, 20, ii. 718.

to sunlight, hydrobenzoin and isohydrobenzoin are formed by simple addition,¹ thus :

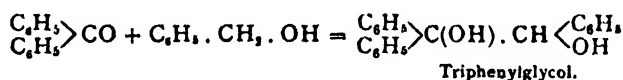


but the action in this direction is far from quantitative and some resin is formed.

Benzophenone and benzylic alcohol interact in a rather more complex way¹; the main product is benzopinacone, formed as follows :

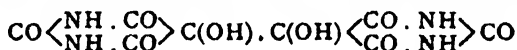


the benzaldehyde formed resinifies in part and is in part converted into hydrobenzoins as above. In addition to these changes, however, benzylic alcohol and benzophenone also give rise to triphenylglycol :

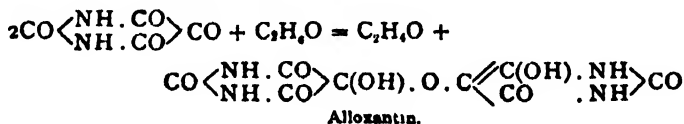


As formic acid is so readily oxidised to carbon dioxide by quinone in sunlight, it might be anticipated that benzophenone would effect a similar change; such, however, is not the case.

The action of ethylic alcohol on alloxan is very striking,¹ alloxantin separating in quantity after a few weeks, the yield after several months amounting to 35 per cent. Aldehyde is also formed, the interaction being similar to that of quinone and alcohol in which quinhydrone is formed; the analogy is strengthened by the fact that, according to Piloty and Finckh,² alloxantin has not the structure that is generally assigned to it,



but bears to alloxan the relation that quinhydrone bears to quinone. The change may therefore be written :



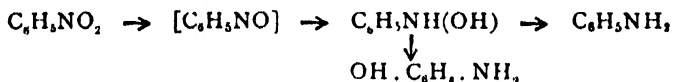
In many of the changes above considered in which alcohol plays a part, moist ether can be substituted for the alcohol with

¹ *Atti R. Accad. Lincei*, 1903, 12, i. 235; *Ber.* 1903, 36, 1575 and 1953.

² *Annalen*, 1904, 333, 22.

advantage. In fact as regards the action of light, the system $(C_2H_5)_2O + H_2O$ seems to be equivalent to $2C_2H_5OH$ but it is more effective and rapid in its action. A striking illustration of this fact is found in the case of phenanthraquinone, which is only slowly affected by alcohol but is decolourised almost instantly when dissolved in moist ether on exposure to sunlight; phenanthraquinol is formed.¹ Isatin also, which is hardly changed by alcohol, gives hydrisatin in ethereal solution.² In these cases acetaldehyde is formed, just as from alcohol itself. The action of *dry* ether, however, is entirely different in character and gives rise to synthetic changes, no aldehyde being formed (see Section VII.).

One of the most complex cases of reciprocal oxidation and reduction is that which occurs when certain benzenoid nitro-compounds are dissolved in a paraffinoid alcohol and the solutions are exposed to sunlight during several months. The case of nitrobenzene has been studied by Ciamician and Silber³ with great care; from their results they conclude that the photochemical reduction takes place in successive simple stages as follows:



Aniline can always be isolated as well as *p*-aminophenol, which, as shown by Bamberger, is undoubtedly a product of the transformation of phenylhydroxylamine; no doubt therefore the latter is formed in the first instance. The fate of the alcohol is uncertain; the corresponding aldehyde can never be isolated. Instead of this, quinaldine (α -methylquinoline) can be separated in the case of ethylic alcohol, 2-methyl-3-ethylquinoline in the case of propylic alcohol and 2-isopropyl-3-isobutylquinoline in that of isoamylic alcohol. These bases are formed in larger quantity than aniline itself, which is the other principal constituent of the basic fraction of the product; and they are the condensation products obtained by heating aniline and concentrated chlorhydric acid with acetaldehyde, propionaldehyde and isovaleraldehyde, respectively, during several hours, in Doebner and Miller's well-known method of synthesising quinoline bases. The formation of the bases is without doubt, therefore, to be

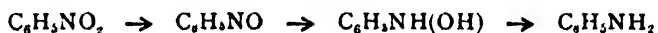
¹ Klinger, *Ber.* 1886, **19**, 1862.

² Ciamician and Silber, *Atti R. Accad. Lincei*, 1901, **10**, i. 92.

³ *Ber.* 1886, **19**, 2899; *Atti R. Accad. Lincei*, 1902, **11**, i. 277, 1905; **14**, ii. 375; *Ber.* 1905, **38**, 3813.

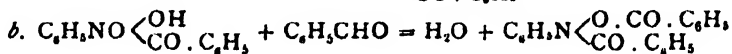
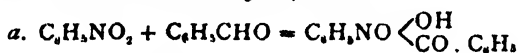
attributed to the primary formation of the aldehyde in each case. In the case of all three alcohols, the reduction of the nitrobenzene is very incomplete, not exceeding 10 per cent. ; it is remarkable that methylic alcohol is almost without action on nitrobenzene in sunlight. The majority of nitro-compounds too are far less affected than nitrobenzene by the alcohols named above. Of the three nitrotoluenes only the meta-compound gives notable quantities of toluidine, whilst *o*- and *m*-dinitrobenzene, the three nitranilines, picric acid and nitronaphthalene remain unchanged.

In the foregoing case of the reduction of nitrobenzene by alcohol, neither nitroso-benzene nor products formed from it, such as azoxybenzene or hydroxyazobenzene, could be isolated ; but when benzaldehyde and nitrobenzene are exposed to light products are obtained which afford proof that in this case the complete series of reduction stages :

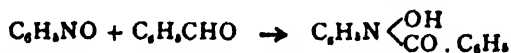


is passed through ;¹ the benzaldehyde is oxidised to benzoic acid. The products actually isolated were benzanilide; benzoyl-phenylhydroxylamine, $\text{C}_6\text{H}_5\text{N(OH) \cdot CO \cdot C}_6\text{H}_5$; dibenzoylphenylhydroxylamine, $\text{C}_6\text{H}_5\text{N(OBz)CO \cdot C}_6\text{H}_5$, and their products of transformation: benzoyl-*o*-aminophenol, $\text{OH \cdot C}_6\text{H}_4 \cdot \text{NHBz}$, and dibenzoyl-*p*-aminophenol, $\text{OBz \cdot C}_6\text{H}_4 \cdot \text{NHBz}$, as well as azoxybenzene and *o*-hydroxyazobenzene.

Ciamician and Silber consider that the principal product, dibenzoylphenylhydroxylamine, is formed directly from nitrobenzene and benzaldehyde, thus :



To understand the formation of the other compounds, it must be admitted that nitrosobenzene is formed initially, $\text{C}_6\text{H}_5\text{NO}_2 + \text{C}_6\text{H}_5\text{CHO} = \text{C}_6\text{H}_5\text{COOH} + \text{C}_6\text{H}_5\text{NO}$ The further action of benzaldehyde on nitrosobenzene gives benzoylphenylhydroxylamine :

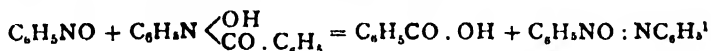


which is in turn reduced by benzaldehyde giving benzanilide :

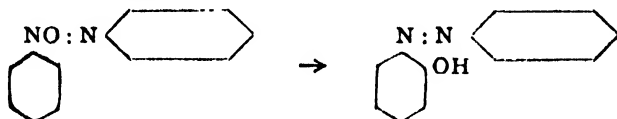


¹ *Atti. R. Accad. Lincei*, 1905, 14, i. 265 ; *Ber.* 1905, 38, 1176.

Azoxybenzene is derived from nitrosobenzene in the manner experimentally demonstrated by Bamberger : in this case by the intervention of benzoylphenylhydroxylamine :



Finally it has been shown by Knipscheer² that azoxybenzene is transformed under the influence of light into its isomeride *o*-hydroxyazobenzene :

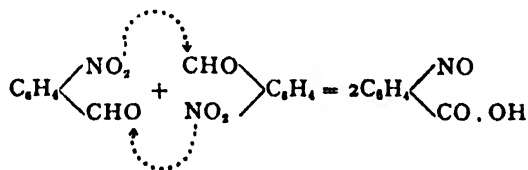


(no *p*-hydroxyazobenzene being formed in this case).

Very few other aldehydes are as active as benzaldehyde in reducing nitrobenzene; anisaldehyde alone produces a similar series of changes. Salicylic, cinnamic and vanillic aldehydes, piperonal and furfural are without action on nitrobenzene and the same is true of the ketones, acetone and acetophenone. A similar series of changes, giving rise to an even more complex set of products, has been observed in the case of nitrobenzene and benzaldehydephenylhydrazone.³

Intramolecular Oxidation

Several remarkable instances have been discovered in which one group in a molecule is oxidised at the expense of another under the influence of light. One of the most striking is afforded by *o*-nitrobenzaldehyde,⁴ which is rapidly changed on exposure to light into *o*-nitrosobenzoic acid. The change may be regarded either as a case of intramolecular rearrangement falling under Class V. ; or as an intermolecular action in which two molecules of the same kind take part, thus :



¹ Angeli's formula for azoxybenzene.

² *Proc. K. Akad. Wetensch. Amsterdam*, 1902, 5, 51.

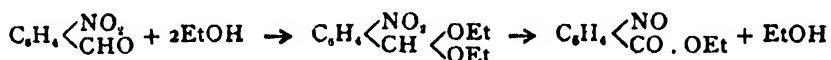
³ Ciusa, *Gazzetta*, 36, ii. 94.

⁴ *Atti R. Accad. Lincei.*, 1901, 10, i. 228 ; *Ber.* 1901, 34, 2040.

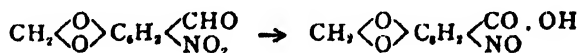
There is no evidence by which the question can yet be decided. In view, however, of the relationship of this change with the interaction of benzaldehyde and nitrobenzene discussed above it will be considered in this section.

The transformation of nitrobenzaldehyde differs from the photochemical changes hitherto discussed on account of the extreme rapidity with which it takes place : whereas the majority of the interactions considered need several weeks or even months for completion and in most cases are very incomplete, the rate at which *o*-nitrobenzaldehyde undergoes change is more nearly comparable with that of ordinary photographic changes. The nitroaldehyde is converted into the nitroso-acid even when in the solid state, the colourless crystals becoming first green but retaining their transparency and finally white and opaque (Bruni and Callegari¹); in solution the transformation of the aldehyde takes place so rapidly that, in a few hours, the tube containing the liquid is full of crystals of the nitroso-acid.

A solution of *o*-nitrobenzaldehyde in ethylic alcohol gives at first ethylic nitrosobenzoate : as an alcoholic solution of *o*-nitrosobenzoic acid does not esterify on exposure to light, the action probably takes place as follows :²



Some *o*-nitrosobenzoic acid, however, is always formed, as in the case of indifferent solvents. Methylic alcohol gives methylic nitrosobenzoate but isopropyl alcohol gives only *o*-nitrosobenzoic acid. The influence of structure is also strikingly shown in the fact that meta- and para-nitrobenzaldehyde do not undergo a similar transformation. Moreover, no such change occurs in the case of *o*-nitrocinnamic aldehyde, $\text{NO}_2.\text{C}_6\text{H}_4.\text{CH}:\text{CH}.\text{CHO}$; but most other ortho-nitro-aldehydes behave like *o*-nitrobenzaldehyde. *o*-Nitropiperonal,³ for example, gives *o*-nitropiperonylic acid :



2:4-Dinitrobenzaldehyde gives *o*-nitroso-*p*-nitrosobenzoic acid⁴

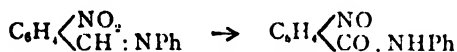
¹ *Atti R. Accad. Lincei*, 1904, **13**, i. 567.

² Bamberger and Elger, *Ber.* 1903, **36**, 3645.

³ *Atti R. Accad. Lincei*, 1902, **11**, i. 277.

⁴ Cohn and Friedländer, *Ber.* 1902, **35**, 1265.

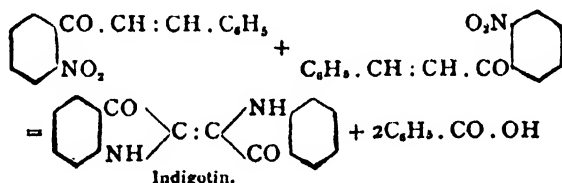
and *o*-nitrobenzylideneaniline is transformed into the anilide of *o*-nitrosobenzoic acid (44 per cent. yield after eight days).



In the same way 4-chloro- or 4-bromo-2-nitrosobenzoic acid can be prepared by exposure to light of solutions of 4-chloro- or 4-bromo-2-nitrobenzaldehyde in benzene,¹ whilst the ethylic salts are obtained on exposing the alcoholic solutions. *p*-Chloro-*o*-nitrobenzylideneaniline in toluene similarly gives 4-chloro-2-nitrosobenzanilide.²

The nitroso-acids themselves undergo further change when exposed to light, giving a complex mixture of products similar to that obtained by Bamberger by the action of aqueous alkalis on nitrosobenzene.³

A very striking instance of intermolecular oxidation-reduction is the transformation of solid benzylidene-*o*-nitroacetophenone into indigotin under the influence of sunlight: the action is as follows:⁴



The nitro-group supplies oxygen for the formation of the benzoic acid.

With these changes may be classed the interesting transformation, under the action of light and traces of iodine, of benzaldehyde into benzylic benzoate, recently observed by Mascarelli and Bosinelli.⁵ This result, which is fundamentally

¹ Sachs and Kempf, *Ber.* 1902, **35**, 2704.

² Sachs and Kempf, *Ber.* 1903, **36**, 3299. Sachs and Sichel, *Ber.* 1904, **37**, 1861.

³ Compare Ciamician and Silber, *Atti R. Accad. Lincei*, 1902, **11**, i. 277. Thus ethylic nitrosobenzoate gives in alcoholic solution mainly ethylic-*o*-nitrobenzoate, diethylic azoxybenzenedicarboxylate, $\text{CO}_2\text{Et}.\text{C}_6\text{H}_4.\text{N}(\text{N}.\text{C}_6\text{H}_4.\text{CO}_2\text{Et})_2$, with some

free azoxybenzenecarboxylic acid and ethylic anthranilate, $\text{NH}_2.\text{C}_6\text{H}_4.\text{CO}_2\text{Et}$.

⁴ Engler and Dorant, *Ber.* 1895, **28**, 2497.

⁵ Gassetta, 1912, **42**, 82.

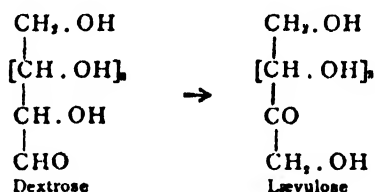
the same as the well-known Cannizzaro transformation brought about by alkalis,



is explained by Mascarelli as involving the formation of benzoyl iodide, $\text{C}_6\text{H}_5 \cdot \text{CO} \cdot \text{I}$, as an intermediate product; although this substance is not formed from benzaldehyde and iodine under ordinary conditions, its production under the influence of sunlight has been actually observed.

Exactly similar changes occur in the case of *p*-tolualdehyde, which gives toluylic toluate when exposed to sunlight.¹

The foregoing cases of internal oxidation-reduction are of special interest when considered in reference to the changes which occur in the foliage leaves of plants. The recent work of Strakosch and others would indicate that dextrose is the first sugar formed in the leaf of the sugar beet by photosynthesis; if this be so, its transformation into lævulose and hence into cane sugar, in which form the sugar is stored in the root, is a change closely analogous with the photo-chemical transformation of the *o*-nitrobenzaldehydes.



II. AUTOXIDATIONS

The part played by light in conditioning the numerous cases of "autoxidation" which have lately attracted so much attention, especially from Engler² and Manchot, has as yet been little investigated. From the point of view of the changes occurring in plants, more especially those brought about by the so-called *oxydases*, such knowledge is particularly desirable. The recent statement of Kernbaum³ that he has observed the decomposi-

¹ Mascarelli and Russi, *Gazzetta*, 1912, 42, 92.

² See Engler and Weissberg, *Kritische Studien über die Autoxydations vorgänge*, Vieweg, 1903.

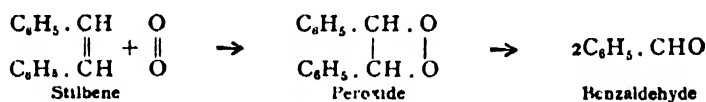
³ *Bull. Acad. Sci. Cracovie*, December, 1911, 583.

tion of water-vapour by sunlight into hydrogen and hydrogen peroxide is suggestive from this standpoint.

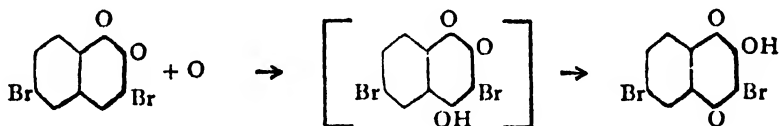
It is well known that aldehydes undergo oxidation in the air, especially rapidly in light, forming peroxides and finally the corresponding acids; benzaldehyde especially behaves in this way. Ketones also oxidise spontaneously in the air under the action of light; acetone, for example, gives a mixture of acetic and formic acids:¹



Unsaturated compounds, such as stilbene,² readily undergo autoxidation under the influence of light; in the case of stilbene, the action occurs even when the solid is left in a desiccator exposed to sunlight, benzaldehyde being first formed and finally benzoic acid, a peroxide perhaps being produced as an intermediate product:



A striking example of a somewhat similar character is that observed by the writer³ in the case of *solid* 3:6-dibromo- β -naphthaquinone, which is transformed almost quantitatively when exposed to bright light into 3:6-dibromo-2-hydroxy-1:4-naphthaquinone (compare p. 266):



The well-known transformation of chloroform into carbonyl chloride under the action of light is a somewhat similar case:



Some striking instances have recently been observed by Ciamician and Silber⁴ of the oxidation of such stable hydro-

¹ *Atti R. Accad. Lincei*, 1903, **12**, i. 235; *Ber.* 1903, **36**, 1575.

² *Atti R. Accad. Lincei*, 1903, **12**, ii. 528; *Ber.* 1903, **36**, 4266.

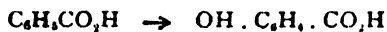
³ *Brit. Ass. Report*, "Isomeric Naphthalene Derivatives," 1902.

⁴ *Atti R. Accad. Lincei*, 1911, **20**, ii. 673.

carbons as toluene, the xylenes and *p*-cymene by oxygen and water under the influence of sunlight. Toluene is converted into benzoic acid (small quantities of benzaldehyde being also formed); the xylenes give the corresponding toluic acids (with traces of the corresponding phthalic acids), whilst *p*-cymene gives *p*-cuminic acid, $C_3H_7 \cdot C_6H_4 \cdot COOH$, together with *p*-hydroxyisopropylbenzoic acid, $OH \cdot CMe_2 \cdot C_6H_4 \cdot CO_2H$ and *p*-propenylbenzoic acid, $CH_2 : CMe \cdot C_6H_4 \cdot CO_2H$.

Formic acid is formed as well in all cases. Oxidation does not take place in darkness. It is interesting that the nitrotoluenes do not undergo oxidation under the same conditions.

Neuberg¹ has recently shown that uranyl salts act catalytically in accelerating the oxidation by air of numerous organic compounds exposed to sunlight; iron salts act similarly.² An especially interesting case of oxidation brought about in this way is that of benzoic acid to salicylic acid:



The autoxidation of menthone is dealt with in Section VI.

III. POLYMERISATION

Among the earliest cases of photochemical action studied were those which involve the polymerisation of unsaturated compounds: thus acetylene is transformed into benzene, brom-acetylene, $CH : CBr$, into *sym*-tribromobenzene, propiolic acid, $CH : C \cdot CO_2H$, into trimesic acid (*sym*-benzenetricarboxylic acid), anthracene into dianthracene (Fritzche, 1866), whilst acridine, anthranol and methylantracene also give rise to polymeric forms. In the case of anthracene, the action is reversible and has been studied by Luther and Weigert³ from the physical standpoint in considerable detail.

Thymoquinone was shown by Liebermann and Ilinski⁴ to polymerise rapidly to dithymoquinone and this change was further studied by Ciamician and Silber in 1886⁵; the conversion

¹ *Biochem. Zeitschr.* 1908, **13**, 305.

² *Ibid.* 1910, **29**, 279.

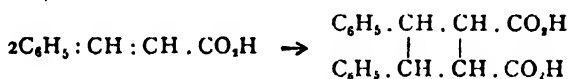
³ Luther and Weigert, *Zeit. physikal. Chem.* 1905, **53**, 416; Weigert, *ibid.* 1908, **63**, 458.

⁴ *Ber.* 1877, **10**, 2177; 1885, **18**, 3193.

⁵ *Gazzetta*, 1886, **16**, 111.

takes place in the *solid* thymoquinone, as when this is spread out on the walls of a large flask (by dissolving in a minimum of ether and then evaporating the solvent) it becomes colourless on exposure to light. The polymerisation does not take place in solutions of the quinone. From the point of view of structure, it is interesting to note that although thymoquinone, $O : C_6H_2MePr^s : O$, rapidly polymerises, *p*-xylylquinone, which differs from it only by containing methyl in place of isopropyl, is not susceptible to light. The same is true of durylquinone, dibromothymoquinone and nitrosothymol, $OH.N : C_6H_2MePr^s : O$.

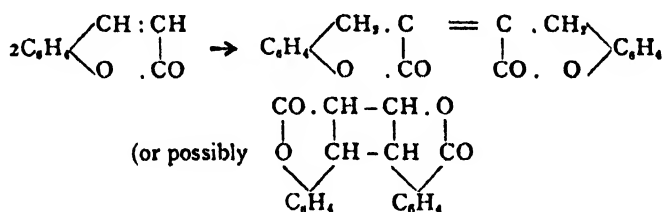
In 1895 Bertram and Kürsten¹ observed that when *dry* cinnamic acid is exposed to sunlight, it is transformed into a dipolymeride, *α*-truxillic acid :



This change, like that of thymoquinone, does not take place in solution, if either alcohol or ether or acetone be used as solvent ; but when cinnamic acid is *suspended* in paraldehyde it is partly converted into its polymer. Stilbene, on the other hand, dissolved in benzene (in absence of air, so as to prevent autoxidation) is converted into distilbene :²



If coumarin dissolved in absolute alcohol be exposed to light³ hydrodicoumarin is formed :



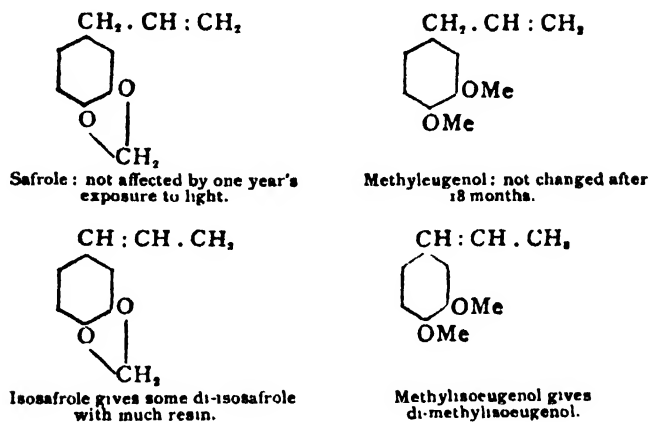
Dibenzylideneacetone, $CHPh : CH . CO . CH : CHPh$, resinifies under the influence of light giving a dimeric form.⁴ The different behaviour of the isomers safrole and isosafrole,

¹ Ber. 1895, **28**, 387.

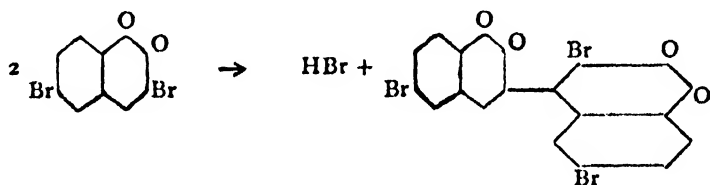
² Ciamician and Silber, Ber. 1902, **35**, 4128 ; Atti R. Accad. Lincei, 1903, **12**, ii. 528 ; Ber. 1903, **36**, 4266.

³ Atti R. Accad. Lincei, 1909, **18**, i. 216 ; Ber. 1909, **42**, 1386.

methyleugenol and methylisoeugenol is shown in the following scheme :



A striking change, allied to polymerisation but accompanied by elimination of hydrogen bromide, is that observed by the writer¹ in the case of 3:6-dibromo- β -naphthaquinone when exposed in certain solvents (ethylic acetate, benzene or chloroform) to the action of light; this substance, which in the *dry* state undergoes autoxidation (see p. 263), in presence of the solvents named is transformed as follows :



Purified benzaldehyde, exposed to light in the dry state during two and a half years, in absence of air, is converted into an amorphous material which is apparently a tetrameric form $(\text{C}_7\text{H}_6\text{O})_4$.²

Small quantities of two *trimeric* forms³ are also produced, one of which was obtained by Mascarelli⁴ as the principal

¹ *Brit. Assoc. Report*, Committee on Isomeric Naphthalene Derivatives, 1902.

² Ciamician and Silber, *Atti R. Accad. Lincei*, 1909, 18, i. 216; *Ber* 1909, 42, 1386.

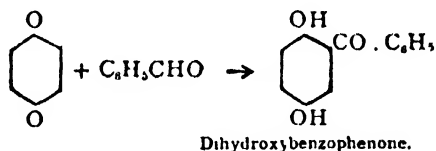
³ *Atti R. Accad. Lincei*, 1911, 20, i. 881.

⁴ *Gazzetta*, 1912, 42, i. 82.

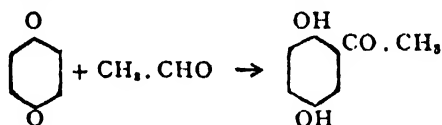
product (together with benzylic benzoate, see p. 261) on exposing benzaldehyde containing traces of iodine to light. In the latter case, only a small proportion of Ciamician and Silber's tetrameric form could be isolated.

IV. CONDENSATIONS AND SYNTHESSES

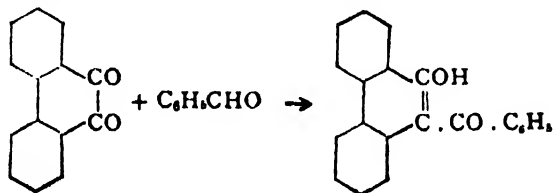
Closely allied with the changes involving polymerisation, that is the union of like molecules, are those in which the action involves the union of unlike molecules. Klinger in 1891 found that benzaldehyde, which easily polymerises in sunlight, also combines readily with other substances, such as quinone, under the same influence. In the case of quinone¹ the action is as follows:



Acetaldehyde, in like manner, combines with quinone to form dihydroxyacetophenone² (acetylquinol):



Phenanthraquinone and benzaldehyde³ interact as follows:



The addition of benzylic alcohol to benzophenone to form triphenylglycol has already been dealt with in Section I. Similar synthetic changes occur in the case of acetone in presence of paraffinoid alcohols such as methylic and ethylic alcohol; as other changes also occur, these cases are dealt with in Section VII.

¹ Klinger and Standke, *Ber.* 1891, **24**, 1340; *Annalen*, **249**, 237.

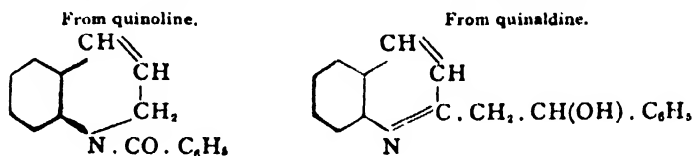
² Klinger and Kolvenbach, *Ber.* 1898, **31**, 1214.

³ Klinger and Standke, *Ber.* 1891, **24**, 1340; *Annalen*, **249**, 237.

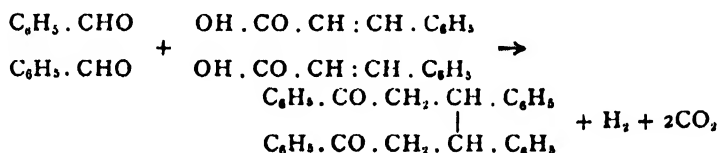
The action of acetone on isopropyl alcohol,¹ however, is simple, consisting merely in a synthetic or additive change, a pinacone being formed:



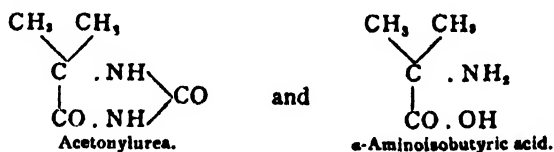
Benrath² found that benzaldehyde combines with quinoline and quinaldine to form compounds of the following structure:



The different character of the action in the two cases is very striking. Benzaldehyde combines with cinnamic acid in still another manner, a complex product being formed by elimination of carbon dioxide and hydrogen:



In view of the production of hydrogen cyanide in the early stages of plant growth, Ciamician and Silber have made a number of experiments on the effect of light on the action of hydrogen cyanide on aldehydes and ketones. Contrary to expectation, the cyanhydril of aldehyde was not affected by light but acetone³ in presence of hydrogen cyanide gave much soluble gummy matter "recalling the peptones in chemical and physical properties" together with α -hydroxybutyric acid, OH.CMe₂.CO₂H, and its amide, OH.CMe₂.CO.NH₂, but as main products:

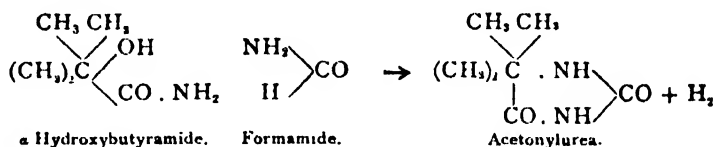


¹ *Atti R. Accad. Lincei*, 1911, 20, i. 714; *Ber.* 1911, 44, 1280.

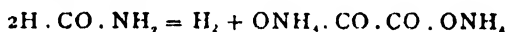
² *J. pr. Chem.* 1906, 78, 383.

³ *Atti R. Accad. Lincei*, 1906, 15, ii. 529; *Ber.* 1905, 38, 1671.

It seems probable that the acetonylurea is formed by the further change of α -hydroxybutyramide under the influence of formamide (produced from the hydrogen cyanide by addition of water, $\text{HCN} + \text{H}_2\text{O} = \text{H} \cdot \text{CO} \cdot \text{NH}_2$), as follows :



The formation of acetonylurea does not take place in darkness ; the way in which the hydrogen liberated according to the above equation is used up is not clear. α -Aminoisobutyric acid is no doubt formed by hydrolysis of acetonylurea. Some ammonic oxalate is also formed in the above case by hydrolysis of hydrogen cyanide (or formamide), a change which also involves liberation of hydrogen.



An interesting synthesis of a substance having alkaloidal properties is described by Paternò and Maselli,¹ who, by exposing acetophenone dissolved in alcoholic ammonia to bright sunlight, obtained a crystalline compound, $\text{C}_{18}\text{H}_{18}\text{N}_2$; apparently, in the formation of this substance, two molecules of acetophenone, two of ammonia and one of alcohol undergo condensation, water being eliminated.

Other syntheses are considered in Section VII.

V. ISOMERIC OR STEREOISOMERIC CHANGE

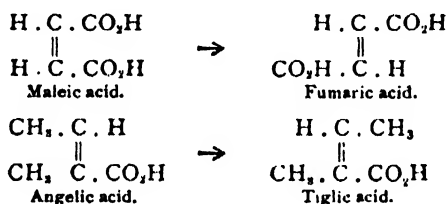
In the case of compounds containing an ethenoid linkage, light brings about very frequently the transformation of one stereoisomeric form into another. Wislicenus² in 1895 observed that in presence of traces of bromine maleic acid is rapidly changed by sunlight into the more stable fumaric acid ; the same change was found by Ciamician and Silber³ to be brought about by sunlight alone in either solid or dissolved maleic acid but to take place more slowly in the absence of bromine. The same is true of the analogous change of angelic into tiglic acid

¹ *Gazzetta*, 1912, 42, i. 65.

² *Ber. Verh. K. Ges. Leipzig*, 1895, 489.

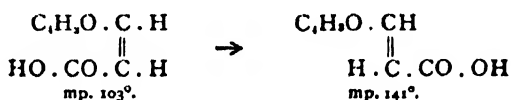
³ *Atti R. Accad. Lincei*, 1903, 12, ii. 528 ; *Ber.* 1903, 36, 4266.

and of isocrotonic acid (liquid) into crotonic acid (solid) (Wislicenus). In all these cases the modification of lower melting-point and greater solubility is transformed by light into the less soluble form melting at a higher temperature. When light is excluded the changes referred to do not take place even in presence of halogen. Thus, for example, when 2 grms. of maleic acid is dissolved in water (5 c.c.) and a little bromine water is added, crystals of fumaric acid separate within a minute when the mixture is exposed to light but in darkness no separation occurs after several hours. Iodine acts far less rapidly than bromine in accelerating the photo-chemical change. The above changes can be expressed by the equations:



Similar results were obtained by Liebermann¹ and may be summarised as follows:

I. *allo*-Furfuracrylic acid \rightarrow Furfuracrylic acid



Change takes place slowly in benzene on exposure to sunlight in absence of iodine. When iodine is present 90 per cent. is converted in sunlight in fifteen minutes; in darkness no action occurs, even when iodine is present.

II. *allo*-Cinnamic acid \rightarrow Cinnamic acid.

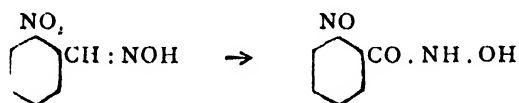


III. The most striking of all is the case of *allo*-cinnamylideneacetic acid, $\text{CHPh:CH:CH:CH.CO}_2\text{H}$. When dissolved in benzene the addition of 3 per cent. of its weight of iodine causes the solution, on exposure to light, to set to a crystalline mass of cinnamylideneacetic acid within three

¹ Ber. 1895, 28, 1443.

minutes; 80 per cent. is converted in this time. In darkness, even when iodine is present, no change occurs in six days. Artificial light, such as that of a Welsbach burner, brings about the transformation but more slowly.

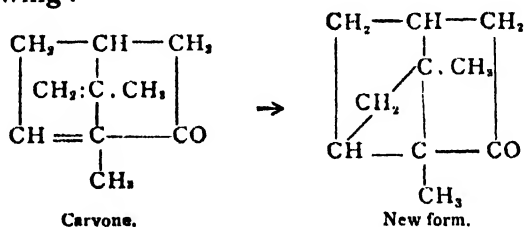
In view of these facts, Ciamician and Silber studied the behaviour towards light of some of the oximes which exist in two stereoisomeric forms; *anti*-benzaldoxime and *anti*-piperonal-doxime are not affected by light but the three nitrobenzaldoximes, *m*-nitroanisaldoxime and chlorobenzaldoxime are each transformed into the stable isomeride of higher melting-point.¹ The case of *o*-nitrobenzaldoxime is particularly interesting, as in view of the transformation of *o*-nitrobenzylideneaniline into *o*-nitrosobenzanilide (observed by Sachs, see p. 261) and of *o*-nitrobenzaldehyde into nitrosobenzoic acid, it was to be anticipated that *o*-nitrosobenzhydroxamic acid would be formed, thus:



Actually this change does not occur.

If a solution of *sym*-tribromodiazobenzene-*syn*-cyanide in benzene be exposed to light, after three days the corresponding *anti*-compound² is formed.

An important isomeric change brought about by light is that which occurs in carvone, which is converted into a well-defined new substance whose exact nature is still uncertain. The change may be analogous with that undergone by the ethenoid compounds considered above but an alternative possibility is the following:³



¹ *Atti R. Accad. Lincei*, 1903, 12, ii. 528; *Ber.* 1903, 36, 4266. Ciusa, *Atti R. Accad. Lincei*, 1906, 15, ii. 721.

² Ciusa, *Atti R. Accad. Lincei*, 1906, 15, ii. 136.

³ Ciamician and Silber, *Atti R. Accad. Lincei*, 1908, 17, i. 576; *Ber.* 1908, 41, 1928.

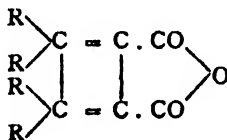
The behaviour of camphor and fenchone is dealt with in Section VI.

The recent results of Stoermer¹ are of great interest in showing that the ultra-violet rays, in many instances, produce effects which are the opposite of those brought about by ordinary light. Thus when the stable, less fusible forms of compounds containing an ethenoid linkage are exposed in benzene or alcoholic solution to ultra-violet rays, they give rise to the labile stereo-isomerides of lower melting-point. The following changes take place from left to right in ultra-violet rays and from right to left in sunlight :

Methylcoumaric acid	\rightleftharpoons	Methylcoumarinic acid.
Dimethyl <i>o</i> -nitrocoumarate	\rightleftharpoons	Dimethyl <i>o</i> -nitrocoumarinate.
Methoxycinnamic acid (or amide)	\rightleftharpoons	<i>allo</i> -Methoxycinnamic acid (or amide).
Fumaric acid	\rightleftharpoons	Maleic acid.

In some instances, as for example the change of cinnamic acid to isocinnamic acid and of methylic coumarate to coumarin, the action is not reversible but takes place in one direction only.

In this place it is not possible to do more than mention the so-called "phototropic" changes studied by Marckwald and others.² Such changes occur particularly in the case of certain aldehyde-phenylhydrazones, which change in colour under the influence of sunlight, the products formed regaining their original colour when kept in darkness. No doubt a change of structure is involved in the alteration. The same is true of the fulgides of Stobbe³ of the general structure :



which undergo similar "phototropic" change. Schlenk and Herzenstein⁴ have observed a somewhat similar transformation

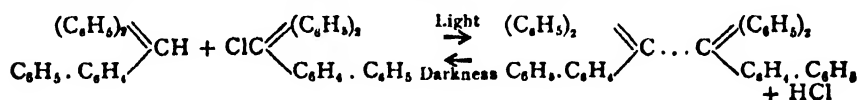
¹ *Ber.* 1911, **44**, 637.

² Marckwald, *Zeit. physikal. Chem.* 1899, **30**, 140; Biltz, *ibid.* **30**, 527; Padoa and others, *Atti R. Accad. Lincei*, 1910, **19**, i. 490; ii. 302; 1911, **20**, i. 675; ii. 712.

³ *Zeit. Elektrochem.* 1908, **13**, 479.

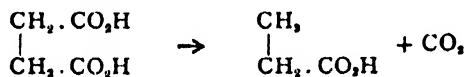
⁴ *Ber.* 1910, **43**, 3545.

in the case of a mixture of derivatives of triphenylmethane with the corresponding triphenylchloromethane, which in sunlight becomes coloured owing to the formation of a triphenylmethyl compound; this in darkness loses its colour owing to the occurrence of the reverse change; for example

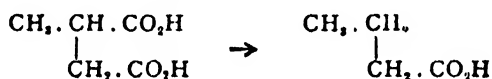


VI. HYDROLYSIS AND RING-SCISSION

One of the best-known cases of decomposition effected by light is that of the paraffinoid carboxylic acids into carbon dioxide and the corresponding hydrocarbon; this change takes place in presence of an uranium salt, which acts as a catalyst. In this way, acetic, propionic and butyric acids give respectively methane, ethane and propane.¹ Succinic acid gives propionic acid:



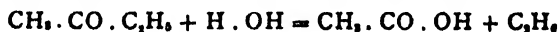
and pyrotartaric acid butyric acid²:



In the absence of such catalysts, light alone brings about striking hydrolytic effects. One of the most simple is the transformation of *wet* acetone by sunlight into methane and acetic acid³:



and of methylethylketone into acetic acid and ethane⁴:



In these experiments, the air must be displaced by carbon

¹ Seekamp, *Annalen*, 1862, 122, 115; Fay, *Amer. Chem. J.* 1896, 18, 269.

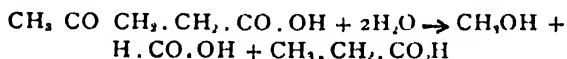
² Wisbar, *Annalen*, 1891, 282, 232. For the analyses of the mixed gases (CO, CO₂, CH₄ and H₂) obtained in the decomposition by ultra-violet rays of many organic substances such as alcohols, sugars, etc., see D. Berthelot and Gaudechon (*Compt. Rend.* 1910, 151, 395 and 478).

³ Ciamician and Silber, *Atti R. Accad. Lincei*, 1903, 12, i. 235; *Ber.* 1903, 36, 1575.

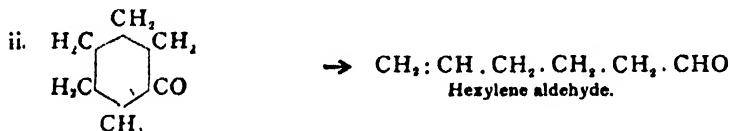
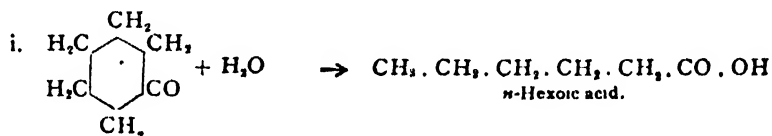
⁴ *Atti R. Accad. Lincei*, 1907, 16, i. 835; *Ber.* 1907, 40, 2415.

dioxide, otherwise oxidation occurs (Section I.); the action is only partial, equilibrium being established when 10 per cent. of the ketone has been transformed.

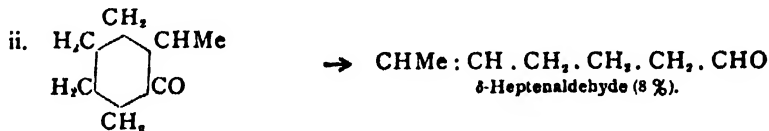
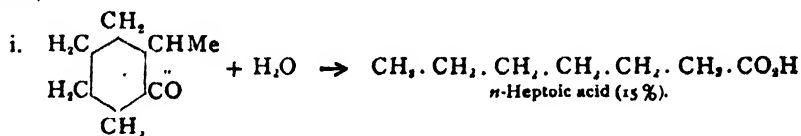
Lævulinic acid is hydrolysed¹ by water (ten volumes) in a highly characteristic manner, methylic alcohol, formic acid and propionic acid being the products, instead of acetic acid and propionic acid as might have been anticipated :



The behaviour of the cyclic ketones when exposed with an excess of water (ten times the weight of the ketone) to the action of sunlight is generally twofold in its character : usually a fatty acid is formed, together with an unsaturated aldehyde. In the case of *cyclohexanone* the changes that occur are :²



o-Methyl*cyclohexanone* under like conditions is changed as follows :

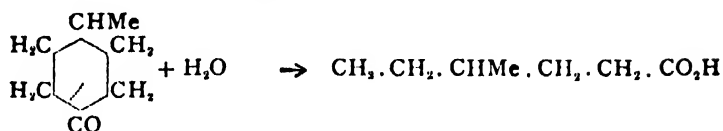


In the latter case, the ring is split only in the manner indicated ; no methyl-*n*-butylacetic acid is formed, as would be the case if splitting occurred between the CO and CH₂, as in the case of *cyclohexanone*.

¹ *Atti R. Accad. Lincei*, 1907, 16, i. 835 ; *Ber.* 1907, 40, 2415.

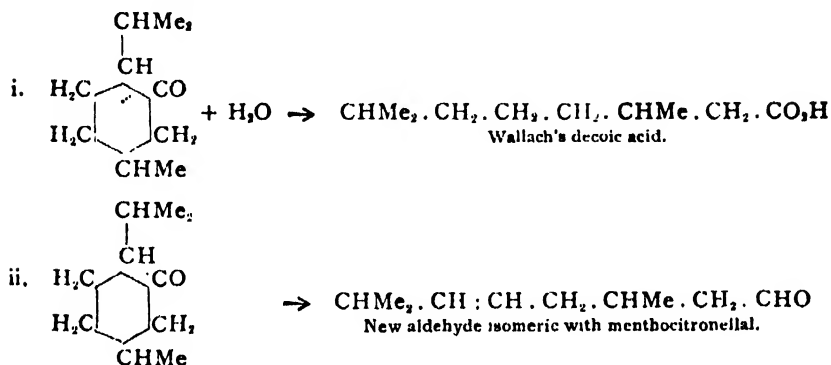
² *Atti R. Accad. Lincei*, 1908, 17, i. 179 ; *Ber.* 1908, 41, 1071.

p-Methylcyclohexanone gives γ -methylhexoic acid :

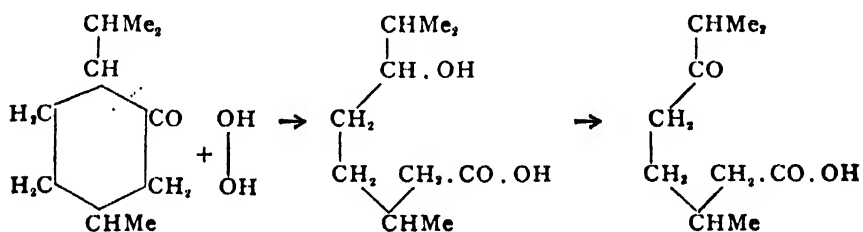


with some heptenylic aldehyde, $\text{CH}_3\text{:CH} \cdot \text{CHMe} \cdot \text{CH}_2 \cdot \text{CH}_2 \cdot \text{CHO}$.

Menthone¹ behaves as follows :



When menthone is exposed to the action of water and oxygen² conjointly, it undergoes oxidation as if it were submitted to the action of hydrogen peroxide :



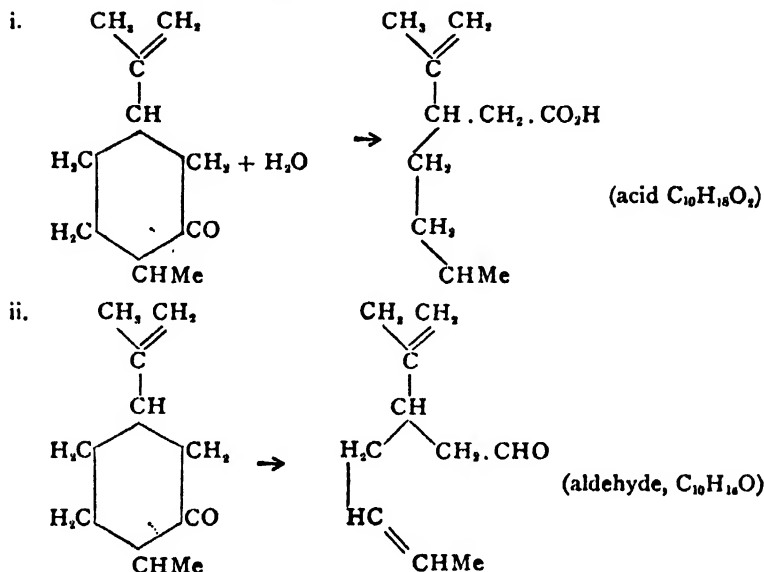
The product is the keto-acid obtained by Arth by oxidising menthol with chromic acid. The ring is split at the same point as when air is excluded.

The behaviour of dihydrocarvone when exposed in aqueous alcoholic solution to the action of light is interesting in comparison with that of carvone, which, as stated in Section V., gives rise to an isomeric form resembling camphor ; dihydro-

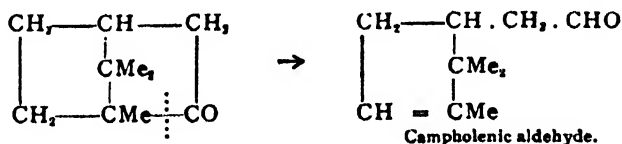
¹ *Atti R. Accad. Lincei*, 1907, 16, i. 835 ; 1909, 18, i. 317 ; *Ber.* 1907, 40, 2415 ; 1909, 42, 1510.

² *Atti R. Accad. Lincei*, 1909, 18, i. 317 ; *Ber.* 1909, 42, 1510.

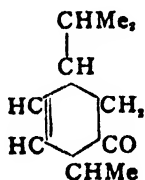
carvone, on the other hand, gives rise¹ to an acid and an aldehyde, the ring being split in the manner already defined:



Camphor² when exposed in aqueous alcoholic solution to sunlight gives a mixture of campholenic aldehyde and a new ketone isomeric with camphor itself. The action which occurs is apparently as follows:



The formula assigned to the new ketone, from its behaviour on oxidation, is



The ketone is the principal product. In this striking case of isomeric change no hydrolysis appears to take place, the

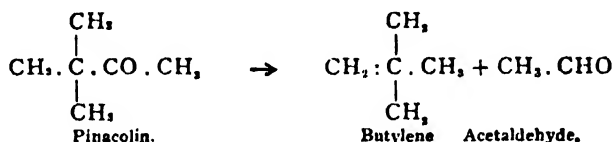
¹ *Atti R. Accad. Lincei*, 1908, 17, i. 576; *Ber.* 1908, 41, 1928.

² *Atti R. Accad. Lincei*, 1910, 19, i. 532.

aldehyde being formed by the scission of the ring in a manner analogous to that which occurs in the cases already considered.

Fenchone¹ is changed under the influence of light more slowly but in a more far-reaching manner, carbon monoxide being evolved and a resinous material formed the nature of which is still uncertain.

The observations recorded on the changes of the cyclic ketones considered above are of special interest from the point of view of the production and transformation of odoriferous principles in plants and the part they play in plant physiology. Among other ketones studied by Ciamician and Silber the following cases may be cited. Pinacolin gives butylene and acetaldehyde:



Methylisobutylketone and the ketone methylheptenone ($\text{CMe}_2 : \text{CH} \cdot \text{CH}_2 \cdot \text{CH}_2 \cdot \text{CO} \cdot \text{CH}_3$) do not undergo change under the influence of light.

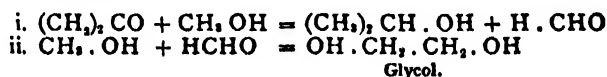
Mention may be made in this section of Neuberg's statement that light is capable of causing the hydrolysis of disaccharides, polysaccharides and glucosides dissolved in water containing 0.5 to 1 per cent. of uranyl salt.²

VII. CHANGES OF COMPLEX CHARACTER

When acetone (one part) mixed with methylic alcohol (two parts) is exposed to sunlight during a year the principal product is one formed by simple addition, viz. isobutylene glycol.³



But isopropylic alcohol (reduction) and ethyleneglycol are also formed, as follows:³

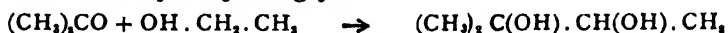


¹ *Atti R. Accad. Lincei*, 1910, 19, i. 532.

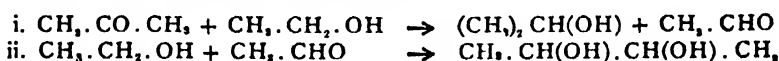
² *Biochem. Zeitschr.*, 1908, 13, 305.

³ *Atti R. Accad. Lincei*, 1910, 19, i. 364; 1911, 20, i. 714; *Ber.* 1910, 43, 945 and 1911, 44, 1280.

In the same manner acetone and ethylic alcohol give as main product, trimethylethyleneglycol :



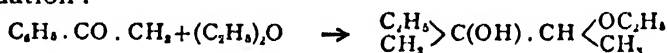
but isopropylic alcohol, acetaldehyde and dimethylethyleneglycol are also formed as follows :



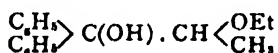
The action of light on the methylic and ethylic alcohol solutions of acetone is in striking contrast with its action on alcoholic solutions of benzophenone or acetone ; in the latter case only benzopinacone or acetopinacone are formed, by a process of reduction.

Acetone and ethylic ether¹ give isopropylic alcohol (reduction) and a compound formed by their association, $\text{OH} \cdot \text{CMe}_2 \cdot \text{CHMe} \cdot \text{OEt}$, together with other substances not yet investigated.

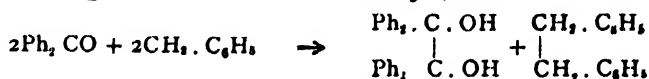
Acetophenone and ether² interact mainly in accordance with the equation :



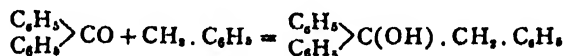
Some resin is formed but no acetopinacone, which is the main product when alcohol is used in place of ether. This behaviour is strikingly different from that of benzophenone which gives with ether much benzopinacone (this is the sole product when alcohol is used in place of ether) together with the compound :



Benzophenone and benzenoid hydrocarbons also interact in a striking manner. When acetone is used much benzopinacone is produced together with some dibenzyl ; thus



but a considerable quantity of diphenyl benzylcarbinol is also formed :

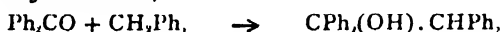


Exactly similar action occurs in the case of benzophenone and ethylbenzene, *p*-xylene and *p*-cymene (in this case the carbinol

¹ *Atti R. Accad. Lincei*, 1911, 20, i. 721. Compare Paternò and Chieffi, *Gazzetta*, 1910, 40, ii. 321.

² *Atti R. Accad. Lincei*, 1910, 19, i. 645. Compare Paternò and Chieffi, *Gazzetta*, 1909, 39, ii. 415.

was not isolated). Benzophenone and diphenylmethane¹ give $\alpha\alpha\beta\beta$ -tetraphenylethanol,



Paraffins and hydrocarbons are transformed by benzophenone in sunlight into unsaturated compounds which then combine with the ketone; the compounds formed are often complex.²

Photochemical action in relation to the refrangibility of the active light. Here it is possible only very briefly to touch upon this question. In an early series of experiments³ Ciamician and Silber, in 1902, showed that the photochemical results they had obtained up to that date were due to the blue-violet rays and were not produced by light of greater wave-length nor by the heat-rays of the solar spectrum. In this respect, the changes recorded were similar to ordinary photographic changes. On the other hand, in the case of chlorophyll, photochemical change is produced chiefly by the rays which are most strongly absorbed; the maximum activity is in the red between the lines B and C, another maximum occurring in the blue near F, with a minimum in the green corresponding with the transmitted rays.⁴ In the case of the red-tinted pigmented cells (Floridaceæ) and yellowish-brown (Diatomaceæ), assimilation was proved by Engelmann to be most active in that coloured light which was most completely absorbed by the pigment of the cell ("chromophyll").

From the recent measurements of Brown and Escombe⁵ of the actual energy absorbed by the green leaf during the period of assimilation it appears that under the most favourable conditions nearly 100 per cent. of the total light-energy absorbed is utilised in bringing about chemical change. The leaf seems, in fact, to be an almost perfect photochemical machine; moreover the photochemical change produced in the leaf differs from all others, not only as regards the enormous amount of energy actually absorbed but in the fact that this energy is mainly taken up from a portion of the spectrum which is usually inactive photochemically: in other words, chlorophyll has properties which distinguish it from most other colouring matters.

¹ Paternò and Chieffi, *Gazzetta*, 1909, **39**, ii. 415.

² Compare Paternò and others, *Atti R. Accad. Lincei*, 1909, **18**, i. 104; *Gazzetta*, 1909, **39**, i. 341, 449; ii. 415; 1910, **40**, ii. 321.

³ *Atti R. Accad. Lincei*, 1902, **11**, ii. 145; *Ber.* 1902, **35**, 3593.

⁴ Engelmann, *Ried. Centr.* 1883, 174.

⁵ Brown and Escombe, *Proc. Roy. Soc.* 1905, **76 B**, 29 (Bakerian Lecture); see also Weigert, *Chem. Wirk. Lichts*, p. 288.

HORTICULTURAL RESEARCH

I. THE PLANTING OF TREES

By SPENCER PICKERING, F.R.S.

MORE than seventy years ago the mind of one of our landowners in England became impressed with our ignorance of the scientific principles on which the greatest industry of the country—agriculture—was based and from small beginnings, with plants grown in pots, this investigations grew till they acquired a home in the *Rothamsted Experiment Station*, the prototype of all the experiment stations which have since been established throughout the world. Following at a humble distance, it was the object of those who founded the *Woburn Experimental Fruit Farm* to attempt for horticulture what Lawes and Gilbert had so ably succeeded in doing for agriculture.

There are few of us who cannot claim to be horticulturists in the limited sense of having grown a few trees or shrubs; even such horticulture must have suggested to those of an inquiring mind innumerable questions as to the why and the wherefore of certain practices which are supposed to be right and of others which are supposed to be wrong. Investigation, however, requires time and money; nothing would have been done in the matter if it had not been that there are still landowners in this country who take a broad-minded view of the duties of their position and of their obligation to the cultivators of the land. In founding the Woburn Fruit Farm in 1894, the present Duke of Bedford was only acting up to the traditions of his predecessors and it may be interesting to record that a hundred years ago a former Duke was intimately associated with an immediate ancestor (Coke of Norfolk) of the present writer in raising the status of agriculture.

Exception has been taken more than once to the locality in which the new station is situated; Kentish fruit-growers, for instance, insisting that it ought to have been established in Kent, growers elsewhere advocating the claims of their own

counties : it is hardly to be expected, however, that any one who takes so much interest in the problems of fruit-culture as to found a station of this sort would establish it on other people's property or anywhere remote from his own observation. Owing to the specialisation to which fruit-growing has given rise, it is a distinct advantage that a general experiment station should not be connected with any particular fruit-growing district : an independent central station, affiliated to subsidiary stations in fruit-growing districts for the study of local problems is, perhaps, the ideal arrangement. It must also be remembered that exceptionally favourable conditions of soil, climate or situation are just as disadvantageous for such stations as are the reverse.

The scientific worker is rarely open to the accusation of ignoring popular beliefs and traditions, for in many cases it is found that these have a solid substratum of truth ; but the well containing this truth is often very deep and requires a deal of clearing out before anything of value is reached. Such beliefs are common with horticulturists, who, as a class, must be reckoned amongst the most conservative of men, ready to adhere to whatever they have been taught in youth, as if it were the accumulated wisdom of ages which no facts or demonstration can upset. With them it is authority, not direct experiment, which must settle disputed points ; a man who has grown trees from boyhood, whose father has grown them before him, is a prophet amongst the people, however limited his intelligence may be. Of this spirit of opposition to inquiry and progress, we have, not unnaturally, experienced the full force, for the Woburn Farm directed its attention, in the first place, to investigating the foundations on which horticultural practice in various particulars was laid and the results in many cases have not been favourable to accepted views.

REPRODUCTION OF FRUIT-TREES

Problems connected with the planting of trees were amongst those to which our attention was first attracted. There are two methods by which a tree reproduces its species in nature, the one by bearing flowers which become fertilised with pollen from the same or from a similar tree, thus producing seed which

will germinate in the ground under favourable conditions and eventually develop into a tree; the other by throwing up from the roots or the base of the stem shoots or suckers which develop into new trunks capable of supplying the place of the original stem when it decays. When the first is followed, the tree produced is a new individual and in the case of cultivated fruit-trees differs materially from the parent tree or trees, generally showing a strong tendency to revert to the original uncultivated type of its ancestors; in the second case, the new tree is really part of the parent and is, in consequence, similar to it in every respect. Most of our cultivated fruit-trees, however, show very little tendency to send up suckers from their roots; similarly, when a twig or young branch is cut from them and planted, this will very rarely root itself and become a tree: consequently other means of multiplying individuals of any particular variety of fruit-tree have to be adopted. The method usually followed, as is well known, is to ingraft a bud or a shoot of the tree required on to some young fruit-tree or "stock," as it is called, already established in the ground; when the bud or buds develop, they reproduce all the main characteristics of the tree from which they were taken, the roots of the stock serving only as a means of conducting moisture and food from the ground to the tree. Yet the character of the root-stock is not entirely without influence on the growth arising from the bud and according as a low-growing bushy tree or a tall growing standard tree is required different root-stocks possessing corresponding characteristics are used. For growing bush-apples and pears, the paradise (*pomme de paradis*) and quince stocks, respectively, are used, as these readily form a mass of fibrous roots which stretch out only a short distance below the surface of the soil; whereas for standard trees the root-stock used is the crab or pear stock, consisting of young trees obtained by sowing pips of the crab-apple or pear. The roots formed by these latter are comparatively few in number but are stronger and penetrate deeper into the soil than those of the dwarfing stock. The general character of the roots of these stock will be evident from the accompanying illustrations. Such stocks, budded or grafted with cultivated varieties of apples or pears, form the "worked" trees which are planted out from one to four years later to form orchards or fruit-gardens.

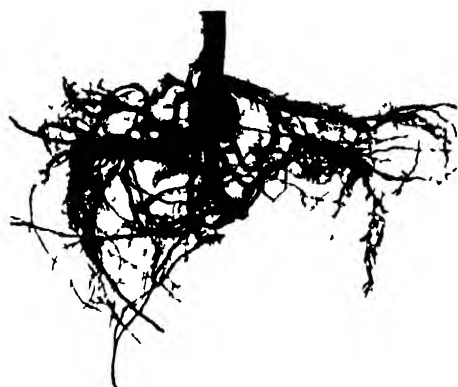


Paradise

FIG. 1



Crab



Quince.

FIG. 2



Pear.

ROOT-GROWTH

A root grows by elongation from the tips and unless such elongation be in progress, either in the root itself or in laterals arising from it, the root ceases to fulfil its functions and the tree dies. The extension of a root depends solely on the presence of certain cells which are capable of multiplying and then elongating; these meristematic cells, as they are called, form a small group situated at the end of the root-tip and are protected from injury by certain outlying cells which constitute the root-cap. The latter are continually rubbed off as the root pushes its way through the soil and are continually reproduced from the region containing the meristematic cells.

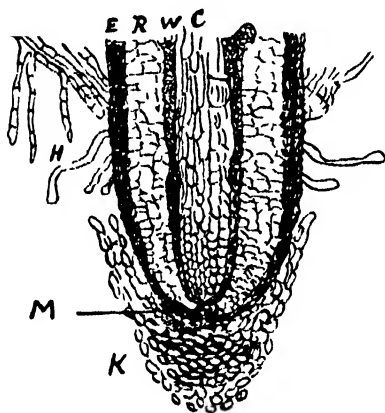


FIG. 3.

C, Central cylinder ; w, Wood vessels R, Cortex ; E, Epidermis ; H, Root-hairs ;
M, Root-tip ; K, Root-cap.

The whole root-tip is very minute, indeed microscopic, so that it is impossible to lift a tree for the purpose of transplanting it without breaking off most of the root-tips; even if they are not broken off, the unavoidable exposure to the air causes them to dry up and to become useless: the continued existence of a tree after transplanting must, therefore, depend on the formation of new root-tips. There are no cells at the cut or broken end of the root able to do this but there are cells situated at intervals throughout the length of the roots which are capable of becoming meristematic and of giving rise to new root-tips, eventually forming new roots branching from the

older one. These may be regarded as analogous to the dormant buds on branches, which show no signs of developing into buds unless the branches are cut back and deprived of those buds which would normally continue the branch-growth. For the development of the dormant root-buds, as they may be termed, intimate contact between the roots and the damp soil is essential; consequently, in transplanting a tree, gardeners always insist on the necessity of getting the earth well shaken in amongst the roots and contend that if the soil be too wet and sticky at the time to admit of this being done planting should not be attempted. This, so far as it goes, is sound practice. A number of trees were planted in soil which was in a good working condition, others in the same soil made unworkable by adding water; these latter made only two-thirds as much growth as the former during the four years following the planting.

RAMMING

But much more intimate contact between the roots and soil can be secured by ramming the soil round the tree, as in fixing a gatepost, especially if the soil be wet at the time: this unusual method of planting, which has so horrified orthodox horticulturists, has been proved beyond question to yield in nearly every case better results than the most careful planting in the ordinary way. Such revolutionary methods of planting were not advocated without ample practical trial. The experiments, which extended over many years, involved the planting of nearly 2,000 fruit-trees and bushes of various descriptions, half of which were planted in the orthodox manner and half rammed: the plantations were made in nearly twenty different soils, ranging from light sand to heavy clay, situated in eight different counties; moreover, the planting was carried out by many experienced planters as well as by the horticulturists at our own farm. The results showed, as might be expected, considerable variation but, on the whole, a very strong balance in favour of ramming: roughly summarised, this was the case in 72 per cent. of the different sets of experiments, whereas in only 11 per cent. were the results somewhat unfavourable, the remaining 17 per cent. being ambiguous. The superior vigour of the rammed trees was manifested in every respect; not only was a greater length of new wood formed in the succeeding year but the shoots were stouter and the leaves larger than



Not trimmed

Trimmed

FIG. 4—Apple Trees



Not trimmed

Trimmed

FIG. 5—Pear Trees

those of the unrammed trees. The superiority in vigour, as measured by the increased growth, amounted in many cases to 100 per cent. and in some cases to a great deal more ; an excess of 50 per cent. may be taken as an average. That this was due to increased root-formation was evident on lifting some of the trees ; instances are given in the accompanying figures.

One great advantage of this method of planting is that it is not a fair-weather method but can be safely practised however wet the soil may be and at a time when planting in the ordinary way would be out of the question. Nor does the ramming consist in merely patting the soil but of pounding it till it is effectually puddled ; so much so that, in our soil, it is quite possible, by stamping with the heel on the ground, to recognise a tree which has been rammed, even two years after the operation. That this consolidation of the soil is a bad thing in itself cannot be doubted and if the whole of the ground were treated in this way the results would probably be fatal ; but in point of fact only a small portion of ground is rammed and the roots soon spread into the looser soil beyond : the only signs of a deleterious effect which have been noticed are that during the first half of the season following the planting the rammed trees are more backward than the unrammed ones, their superiority not asserting itself till the end of the first season, in some cases not till the second season. In one instance only has ramming proved disastrous, that was in the case of some trees planted in the London clay, at Merton, where the absence of aeration affected the soil so much that it became quite black and gave off hydrogen sulphide, which killed the trees. In other heavy and clayey soils (the Woburn farm itself is situated on the Oxford clay) no such deleterious effect has been noticed and in most cases the beneficial results of the ramming in heavy soils have been conspicuous. On the other hand, in light sandy soils ramming has no effect, for the simple reason that any consolidation of the soil effected by this operation will have disappeared before the tree starts into growth in the following spring.

DAMAGED ROOTS

In thus roughly ramming a tree into the ground some mechanical damage must often be done to the roots ; but this is of little or no consequence, as may easily be realised when

it is remembered that the life of the tree depends on the formation of new roots, not on the preservation of the old ones. Each item of damage and of supposed bad practice in planting trees has been made the subject of separate experiments. The general result of these has been to show that a certain amount of damage to the roots is actually beneficial. It is not very difficult to see the reason of this, for such damage generally results in the new roots being formed from the thicker and stronger parts of the old roots where the store of reserve food is greater, so that the new roots develop more vigorously. Thus, we have found the shortening of the old roots to different extents to be of some slight advantage, so long as not more than one-third of the whole length is removed: greater shortening is detrimental. In the same way, the removal of all the smaller roots of a less diameter than 2 mm. is found to be beneficial (plums and pears were investigated) but loss in vigour has followed the removal of those up to 4 mm. Most of the smaller roots, under ordinary conditions, become too much dried up to recover their functions after the tree has been replanted, consequently they die off: this has been fully established by marking these roots by tying pieces of silk round them and lifting the tree again at the end of the first season: it is easily intelligible, therefore, that the tree will be benefited by the removal of rootlets which in any case will decay. Nothing more futile can be imagined than the way in which gardeners carefully spread out and tend these fibrous roots which are already virtually dead. However, the store which is set on a tree which has a mass of fibrous roots has this justification, that if a tree has sent out a good mass of roots while in the nursery it is probable that it will do so again after replanting in the orchard.

That the spreading out of the main roots and avoidance of all injury has any beneficial effect has been disproved by actual trial. In some cases trees were planted with their roots bent, twisted and tied together tightly in a ball under the trees, whilst in others the roots were lacerated to an extent far in excess of any probable accidental laceration. It was found that the number and vigour of the new roots formed was practically unaffected by this treatment and that there was no detrimental effect on the tree; even slight benefit accrued in some cases.

Another point in which accepted rules find no justification in

practice is in the careful trimming of the broken ends of roots, which is supposed to be essential, even to the extent of laying down the law that the cut must be made in a certain direction. This is founded, no doubt, on the erroneous idea that the cut end of a root will grow when the tree is replanted, which it cannot do for the simple reason that there are no meristematic cells there capable of forming a new root-tip. Nor even do the majority of new roots form near to the ends of the old ones: in a large number of cases which were investigated, using apples on the paradise stock, it was found that only 15 per cent. of the new roots formed within a quarter of an inch from the old root-ends, a like number started from the stem itself, the remaining 70 per cent. arose from the other parts of the main roots. Moreover a long straggling root will often fail to send out any new rootlets, the root eventually dying in consequence; or if rootlets are sent out from near the end, these are of a feeble character. Instances of this may be noticed on examining Figs. 4 and 5. Whether the end of a broken root be trimmed or not appears to make no difference to the welfare of the tree and to affect only slightly the new root-formation from the particular root which is broken or cut, the breaking, instead of cutting the root, being merely tantamount to a little extra shortening.

It has thus been found that all the practices which are so strenuously advocated as essential to the proper planting of a tree are for the most part immaterial and may, even with some slight advantage, be violated; whilst as regards one of them, ramming, the advantage in such violation is very considerable: and this novel practice in planting is not only borne out by strict experiment but is the rational consequence of what is known as to the way in which roots are formed. It must be admitted, however, that these experiments were not originally based on any views as to what ought to be but arose from an endeavour to demonstrate the necessity of following all the rules prescribed for the "proper" planting of a tree. One set of trees was planted with all the customary rules violated by way of object lesson; but instead of suffering from the treatment they received, they flourished better than their carefully planted neighbours. The results were set aside as accidental and fresh plantations made in a similar way: this course was repeated four times during six or seven years but always with the same result, so that the fact had, perforce, to be accepted. Other more

specialised experiments followed which served to account for the results on the lines explained above.

THE PROPER DEPTH FOR PLANTING

Two questions of considerable importance remain to be noticed in connexion with planting. These are the depth at which a tree should be planted and the preparation of the soil before planting. A safe practical rule is to plant the tree at the depth at which it was growing in the nursery, this being easily recognisable by the mark of the earth on the stem. In some cases, however, purely for experimental purposes, we planted trees with their roots buried to a much greater depth and were surprised to find that these flourished much better than those planted at the ordinary depth. These trees, however, were not fruit-trees in the horticulturist's acceptance of the term but young paradise stocks. The accompanying illustrations will show what had happened and will serve to explain the apparently anomalous results. In Figs. 6 and 7 are shown six of the stocks as they were before planting and the same stocks as they appeared when lifted two years afterwards. In this case they had been planted with their roots 6 inches below the ground-level, this being indicated by the horizontal line in the figures. The root-system at the end of the two years is practically the same as that in existence at planting but more developed. Fig. 7 represents six trees planted with their roots 24 inches below the surface; in this case the behaviour of the trees has been very different: the original root-system has not developed and in most instances has visibly shrunk, these roots and a portion of the stem above them gradually dying; but in their place there has arisen from the stems higher up a new root-system and the new roots composing it, having found abundance of stored material for their nourishment, have developed strongly and, as a consequence, the growth of the branches, also, has been much more vigorous than in the case of the trees planted at the ordinary depth. Similar, though less marked, effects followed when the trees were planted 12 inches below the surface.

These experiments, by illustrating the vigour of new rootlets arising from the thicker root-bearing portions of a tree, have an important bearing on the explanation already given of the results of careful and rough planting but they must not be interpreted as showing the advisability of planting an ordinary

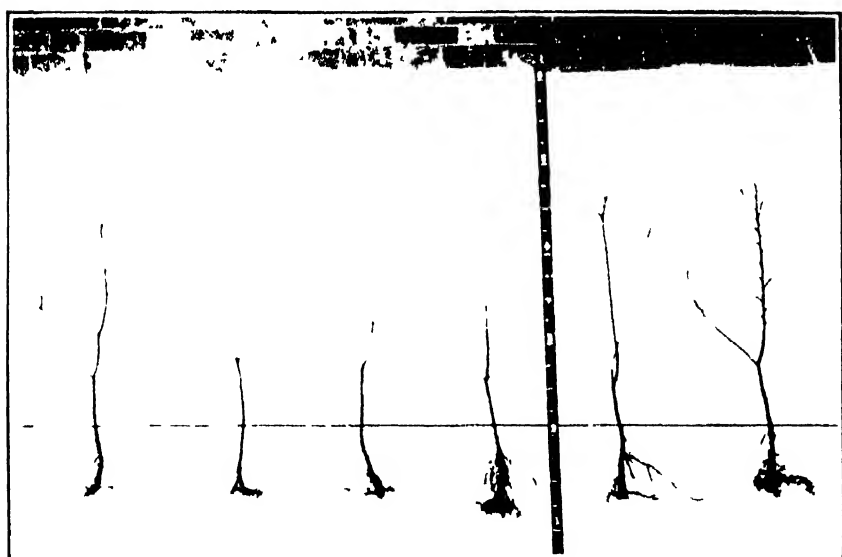
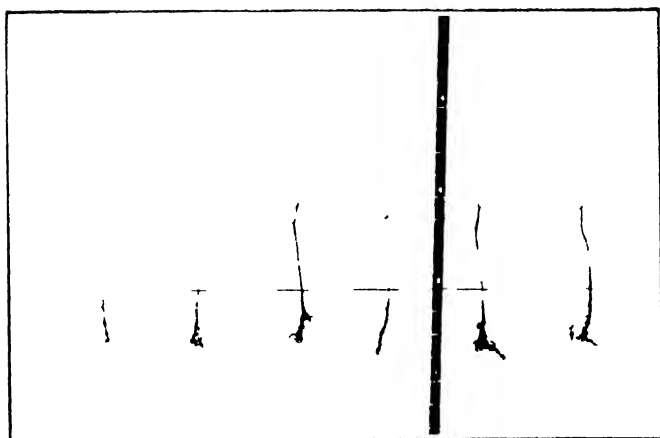


FIG. 6.

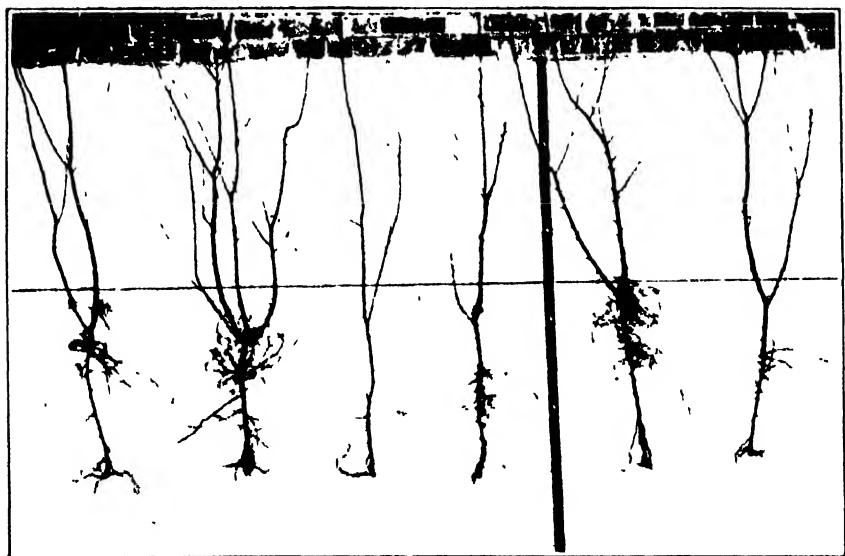
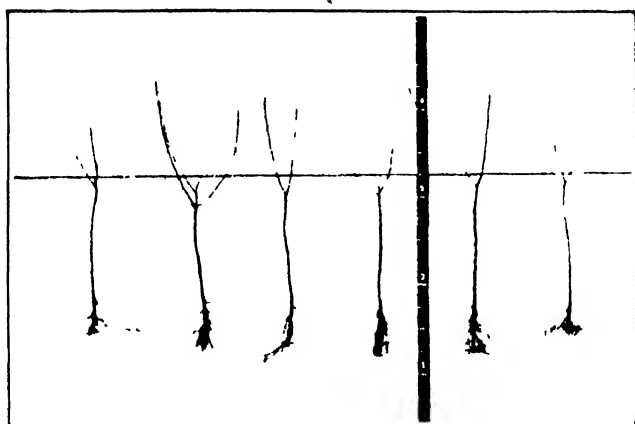


FIG. 7.

"worked" fruit-tree at this depth in the soil. Quite the contrary. The beneficial effects observed were due entirely to the fact that these paradise stocks were capable of throwing out new roots from their stems, whereas the stems of ordinary apple-trees have in most cases no such power, so that if buried in the way described, the original roots would die off as in the case of the paradise stock but no fresh root-system would be formed in substitution. Even when crab stocks were used in place of paradise stocks, the results were found to be unfavourable, for the crab stocks do not throw out roots as easily as do the paradise stocks. It may be noticed, too, that the behaviour of the individual paradise stocks varies considerably, one of the six shown in Fig. 7 having made very little growth, because the stem, for some reason or other, was incapable of producing new roots.

It is evident from these results that there is a particular depth below the surface which is the most favourable for root-formation. This must vary with the nature of the soil and with the habit of the plant but will generally be from 6 to 12 inches below the level of the soil. This, as a rule, will be the best depth at which to plant a young tree; but small variations of, for instance, 4 inches in either direction have been found to be quite immaterial, for in such cases the new roots that are formed have no difficulty in making their way to the level at which they flourish best.

High planting, in another sense, is sometimes adopted, the roots being placed at the ground-level but covered up with earth in the form of a mound 6 inches or more high. This is advantageous if planting has to be done in a wet locality. At present we are investigating its effect as a means of minimising attacks of canker: so far the practice seems to have led to good results from this point of view but it would be premature to draw any definite conclusions yet. As to its effect on the general behaviour of the tree, this varies with the season, being, as might be expected, good in a wet season and bad in a dry one. No one, of course, would think of planting a tree in this way in a light sandy soil.

AERATION OF THE SOIL

The depth at which roots will flourish best is dependent, no doubt, on the conditions prevailing in the soil with respect to air and moisture. Aeration is necessary for the oxidation of organic matter in the soil and of that thrown off from the roots, the

carbonic acid thereby produced playing an important part in rendering the mineral constituents of the soil soluble and assimilable by the plant; aeration is also necessary for the existence of the bacteria on which the plant is dependent for its supply of soluble nitrogen. The importance of an air-supply to the roots is rendered evident by the failure to grow plants in water unless this be well aerated; also, no surer way of damaging or killing trees exists than that of allowing the soil to become water-logged while they are in active growth. Thousands of trees were killed in this way during the wet summer of 1903. On the other hand, it is surprising how limited the supply of air to the roots may be without interfering materially with the growth of a tree. In some experiments at Woburn a number of apple-trees were each surrounded by an iron drum, 3 feet in diameter, 18 inches deep; when this had been driven down into the soil, the soil within the area enclosed by the drum was covered with a 2-inch layer of cement. Each tree was thus enclosed in a sort of tub and its roots could only obtain such moisture and air as permeated through the stiff clay subsoil 18 inches below the surface. Yet these trees flourished during four years just as well as and even slightly better than similar trees which were not enclosed; and though afterwards they began to fall behind-hand, owing to the exhaustion of the limited amount of soil available for their growth, they are still—after thirteen years—fairly healthy trees. Trees planted in towns, often with their roots covered by paving-stones, afford familiar instances of the extent to which they will thrive with a very limited access of air to their roots. One very striking illustration may be noticed just outside St. Pancras Station. The Midland Railway line passed over a burial-ground in which there were some trees with stems up to about a foot in diameter. This burial-ground was done away with about twelve years ago and the ground made up to the level of the railway line by dumping on to it some 13 feet of earth and rubbish: the trees were, consequently, buried to this depth, leaving only their heads above ground: yet they have continued to live and are still in a fairly flourishing condition.

TRENCHING

The question of trenching or double-digging the soil preparatory to planting fruit-trees is one of considerable importance

to growers, as it is a costly operation, especially in stiff soils where it may be expected to do most good. The trenching usually adopted (bastard trenching) consists in digging and moving the first and second depths or spits of soil and breaking up but not removing the third spit ; the second and first spits are then put back into their original position. Ploughing with a sub-soil plough is sometimes substituted for trenching. In vegetable growing, trenching is generally understood to mean more than this, a liberal supply of dung or refuse being buried in the trench drawn out in the course of the digging ; this materially alters the character of the soil. Trenching in its strict sense has alone been investigated. The investigation embraced five instances in different soils, fruit-trees being planted in the trenched and untrenched ground and their behaviour examined. At the same time the alteration effected in the soil by the trenching was investigated by Dr. E. J. Russell, who determined the water and nitrogen present in the various cases. The results have not been quite completed yet but they are sufficiently advanced to show that trenching has very little effect, when measured either by the behaviour of the trees or by the alteration in the soil. In many cases the effect has been *null* and whether it be appreciable or not seems to depend chiefly on the character of the seasons following the trenching. In any case, the beneficial effect is much too slight to compensate the planter for the cost of the operation.

THE RELATION OF MIND AND BODY¹

By J. S. HALLDANE, M.D., LL.D., F.R.S.,

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FROM our everyday standpoint a man or higher animal is a personality consciously and purposively controlling, with a certain amount of success, a surrounding physical environment. On closer examination, however, this conception appears unsatisfactory: for the reactions between his body and the environment are apparently physical and chemical in nature: the body itself is apparently part of the physical and chemical world; the changes within it are apparently physical and chemical changes, no break being noticeable indicative of any point at which they are controlled by an independent mind or soul. Consciousness seems, therefore, to be nothing but an accompaniment of physical and chemical changes within the body.

The facts on which this conclusion depends appear at first sight to be unassailable and to become more and more cogent with every year of advance in physiological knowledge. Psychologists thus tend to be driven into the position which has come to be known as "parallelism" or "epiphenomenalism."

It is important to point out, at the outset of the discussion, that if once we admit that the living body, whatever its peculiarities, either forms part of or exists in a real physical world of matter and energy, we are inevitably committed to the conclusion just indicated: for we can proceed to demonstrate experimentally that the admitted physical and chemical conditions determine all bodily activity, conscious or unconscious: we can trace all perception and memory to the action of physical stimuli; and we can show that the working of the brain depends on physical and chemical conditions. Cut off the oxygen supply to the brain even for a few seconds and all evidence of consciousness disappears completely, only reappearing again if

¹ A contribution, with some additions, to a discussion in the Physiological Section of the British Association meeting at Dundee, 1912.

the supply be quickly restored. Make some other minute alteration in the chemical composition of the blood and a man's behaviour is completely altered: he may be reduced to below the level of a beast. We are in this way forced to admit that if there be a soul, all its manifestations are dependent on physical conditions; and this being so, it seems scarcely worth arguing whether, as the vitalists and (to use Dr. McDougall's term) "animists" maintain, there is something else in a man or animal apart from physical phenomena mysteriously accompanied by gleams of consciousness.

It is the premises of this argument which I wish to examine; indeed there must be examined with the utmost care if ever the two sciences of biology and psychology are to be set on a firm theoretical basis. Living, as we do, in a time when physical conceptions are on all hands tacitly or explicitly assumed to correspond to the reality of our visible universe, it is difficult to obtain a popular hearing for any doubts on the subject; and even from the philosophical side there comes the argument that, unreal in ultimate analysis as the physical universe is, physical conceptions are nevertheless the forms under which alone such knowledge as we possess is possible.

Now it seems to me very clear that in the case of living organisms and their physiological environment, we cannot express the observed facts by means of physical and chemical conceptions but must and do have recourse to the conception of organic unity; and must use this conception as our fundamental working hypothesis just as the physicist uses the conceptions of matter and energy. This means nothing less than a definite break all along the line, including the environment, with the purely physical conception of nature. We may, it is true, endeavour to give a physical description of the phenomena of life; but such attempted description cannot express the main facts.¹ The time at my disposal does not

¹ In his Address as President of the British Association Prof. Schäfer deals with "The Nature, Origin and Maintenance of Life" and defends the thesis that "the problems of life are problems of matter." Needless to say, I am unable to accept his general conclusions. It appears to me that he has failed to apprehend correctly the general trend of biological advance, particularly during the last fifty years; and that he completely ignores the fundamental difficulties involved in a physico-chemical conception of life. Living organisms are distinguished from everything else that we at present know by the fact that they maintain and reproduce themselves with their characteristic structure and activities. Nothing

allow of my developing this position here; but I may perhaps refer to my address as President of the Physiological Section of this Association in 1908. I will only venture to remark that the position indicated involves a far more thorough departure from mechanical explanations than that of the old vitalists or their more recent representatives, although I am in agreement with the position of the vitalists in their main criticisms of what may be called, for the sake of shortness, "mechanistic" biology. The vitalists cut the ground from under their feet by accepting the physical conception of both the environment and the body substance; they cannot consistently escape from the consequences of this acceptance. The conception of organic unity implies a biological, as distinct from a physical, interpretation of environment as well as organism; and the biological interpretation is natural and necessary where biological facts are concerned. The physical and the biological interpretations are each theoretically applicable to the whole of Nature; but neither can be actually applied completely, as only part of the known facts correspond in either case. I feel no personal doubts that the mechanistic biology, in spite of the great names associated with it, including that of the distinguished President of this Association, will soon be a thing of the past.

The conception of organic unity applies to the whole of what may be called the "vegetative" aspect of life but takes us no

resembling this phenomenon is at present known to us in the inorganic world; and if, as we may confidently hope, similar phenomena are ultimately found in what we at present call the inorganic world, our present conception of that world as a mere world of matter will be completely altered. Prof. Schäfer points to the numerous physical and chemical processes which we can distinguish by abstract thought within the living body; he completely ignores the *actual* fact of their maintenance in organic unity. The more detailed and exact our knowledge has become of the marvellous intricacies of structure and function within the living body, the more difficult or rather the more completely impossible has any physico-chemical theory of nutrition and reproduction become. The difficulty stands out in its fullest prominence in connexion with the phenomena of reproduction and heredity. I can find in Prof. Schäfer's address no serious attempt to deal with this difficulty. He has much to say of the physics and chemistry of colloid nitrogenous material and he makes play with the obsolescence of the distinction formerly drawn by chemists between "organic" and "inorganic" chemistry; but he ignores the evident differences between living organisms and non-living material whether "organic" or "inorganic," colloid or crystalloid. He also fails to see what constantly strikes me in my work as a physiologist, that the advance of biology is everywhere hampered and confused by the physico-chemical theory of life.

further and no higher. A mere organism, regarded simply as such, fulfils its biological destiny blindly and without evidence of consciousness; and just as physical conceptions are inadequate to express the phenomena of vegetative life, so are biological conceptions inadequate to express the phenomena of conscious existence.

What characterises any distinctively physiological or biological phenomenon is that whether it relate to the body or to the environment it can only be interpreted as an element in an organic whole constituted by the life of the organism. Nevertheless much that we find in the living body and most that we find outside it cannot be interpreted as organically determined. The advance of biology is constantly increasing the sphere of the organic at the expense of the apparently inorganic; but the sphere of the inorganic increases just as rapidly.

In conscious life there comes in a quite new factor: for an organism which perceives and wills, however dimly, is taking into the unity of its own life the inorganic element. What is perceived or willed is outside mere organic life and yet has a determination or meaning in relation to the past, present and future of the organism and cannot be adequately expressed as a mere physical event. Perception and volition are always "practical": their nature can only be expressed as elements in the teleologically determined whole of a conscious personality. It does not matter whether we approach this fact from the psychological or the physiological side. From the psychological side an isolated sensation or element of whatever kind in consciousness is a meaningless abstraction: from the physiological side an isolated physical stimulus or concomitant of sensation is equally meaningless. When we speak of localisation of sensation we are only repeating empty words. The theoretical basis of physiological psychology as ordinarily understood is wholly unsatisfactory. We have scarcely even reached the threshold of a true physiological treatment of the central nervous system: for the present we have to content ourselves with a crude physical treatment of the subject, in which physical metaphors are everywhere substituted for experimentally ascertained facts.

The distinctive behaviour of men and conscious beings in general cannot be interpreted except in terms of conscious personalities living in an environment of their own percepts and acts, which has grown with them and exists for them. In other

words persons are real and no mere walking automata or automata controlled by souls. The reasoning to the contrary is based on the *petitio principii* that the physical interpretation of the universe corresponds fully with reality. The physical world is taken to be real by itself, though it is only real as part of a known world and as no mere "unearthly ballet of bloodless categories" but the expression of concrete living personality. It is on the basis of abstracting from the primary fact that the physical world is known, that we build up an impossible theory of the rest of our experience—impossible because it can give no account of life or of knowledge and volition. It is only for our own practical purposes that we separate off the physical world from its relation to ourselves as the subjects for whom it exists; and the confusion arises from our forgetting this fact. In the argument that all the conscious behaviour of a man or animal is ultimately dependent on physical and chemical stimuli from the environment, acting on the physical and chemical structure of the body, the whole question is begged from the outset; for the assumed physical stimuli and physical structure do not behave as such; the facts do not fit into the assumption we have made as to their nature. Stimuli and structure possess alike a meaning—a determination as part of the unity which we recognise as personality. We cannot separate the stimulus from the consciousness of it. We are in presence of something which cannot be expressed in physical terms. No amount of tracing of paths of nervous connexion or localisation of function will help us to a physical analysis of the unity of personality, because the unity determines the whole and includes the environment.

It is none the less true that apart from all attempts at a physical analysis of personality, there is abundant room for purely physical and physiological investigation of living organisms, provided that it be clearly recognised that in these investigations we are for our own practical purposes deliberately leaving out of account certain aspects of the facts we are investigating. This is, indeed, the case in all scientific investigation, whether mathematical, physical, physiological or psychological.

We can, for example, proceed to measure, weigh and describe in physical or chemical terms anything in connexion with the living body; but when we look closely we soon see that

our data are, at best, of only a limited practical value. If we weigh an animal or man, we obtain data which may be of great practical value; but what are we weighing? It is not the living body, because it includes the contents of the alimentary canal and other cavities and perhaps the clothes: it also includes deposits of fat, water and other material stored in the body, either within or outside of living cells: also liquids such as the blood plasma and lymph-deposits, of inorganic matter in the bones and apparently lifeless organic matter in the connective tissues and elsewhere. When we investigate metabolism or chemical constitution of material or any other process or state occurring in the body, similar questions have to be faced; and we begin to realise that in investigating biological questions from the standpoint of physics and chemistry alone we are dealing with a collection of abstractions from reality and that we can do better by using a less abstract working hypothesis.

These physical investigations, like all scientific investigations, have nevertheless a very great practical value: for though they are partial and one-sided they give us the best insight we can for the time get as regards countless matters of detail in our experience. The great mistake, leading to such conclusions as that living organisms are physico-chemical mechanisms or that conscious behaviour is nothing but physico-chemical change accompanied by consciousness, is to lose sight of the wider point of view which shows us that in physical or indeed any scientific investigation we are always dealing with partial aspects of reality.

We can arrange the sciences in a certain order, according as they deal with more or less abstract and one-sided aspects of reality. The purely mathematical sciences come lowest in this order; next to them come the physical sciences; then biology; whilst psychology and ethics deal with what is least abstract. But if the mathematical sciences stand lowest in one way, in another way they stand highest, as they have the widest and most general field of application; and all knowledge and practice involve quantitative treatment.

Between body and mind there is no interaction, simply because the body, more fully understood, is the mind. From the physical and chemical standpoint a man is about 70 kilogrammes of material with a certain configuration, properties and internal movements: this material consisting of a great variety

of chemical compounds, interacting upon one another in various ways. From the physiological standpoint the man is a living organism blindly fulfilling its biological destiny. From the psychological standpoint he is a person, the subject of purposive knowledge and volition. The man as mere physical body or organism is an evident fiction or abstraction from reality, though a very necessary one for our imperfect knowledge. As a conscious individual personality he is at least far less of a fiction.

The physical sciences, biology and psychology, go on their several ways, accumulating knowledge which each science interprets according to its own working hypotheses and subject to the limitations of these hypotheses. Each lower science also hands on what is relatively speaking raw material to the higher one. The attempt to resolve the higher into the lower, as by making mind dependent on body, is, however, foredoomed to failure.

To sum up, the relation of body to mind is not that psychical phenomena are the mere accompaniments of physical processes in the body nor that there is interaction between body and an incorporeal mind or soul but that body is conscious personality looked at incompletely or abstractly. In other words, conscious personality is the truth of the body and its environment ; and the physical causes which seem at first sight to determine the mind are only superficial appearances. This is merely another way of saying that however little we understand it in detail our world is a spiritual world.

We are not thereby committed to the absurd position that the personality of the universe is a man's own individual personality coming into existence at a certain date and disappearing again at a certain other date. Just as biological facts have taught us that the life of each individual cell or organism is only part of a wider life, so have ethical and religious facts shown that the individual personality in its full realisation is the expression of divine personality, which alone can be the ultimate truth of all existence. The individual personality, including his ideas of the world and his ideals of conduct, is evidently a "product of his time"—the expression of a wider personal life which he only realises in living it and living it whole, confident in his participation in it and ready to give up his mere individual interests or even his life itself should his duty lead him to do so.

In drawing these conclusions I am only following on the lines of great philosophers who have reached essentially the same results. It is unfortunate that owing to faults on both sides there has in recent times been so little real contact between natural science and philosophy; but I hope that this discussion in an assembly of men of science may prove a step in promoting closer relations in future and once more bringing these two great branches of human knowledge and endeavour into living connexion.

SPECULATIONS ON THE ORIGIN OF LIFE AND THE EVOLUTION OF LIVING BEINGS¹

BY E. A. MINCHIN, F.R.S.

ANY statements that can be made concerning the Origin of Life must be, at the present time, of a purely speculative nature—a speculation being defined as the logical process of drawing from established data certain conclusions which cannot be directly verified. Though the degree in which a speculation approximates to the truth and commands our confidence in any given case will depend entirely upon the nature of the evidence by which it is supported, a proposition which cannot be directly verified may nevertheless be based on evidence so strong that it receives unhesitating assent from those who are able to understand the premises and follow the reasoning. At the opposite pole to such conclusions are those which cannot be either proved or disproved and are therefore valueless; as if, for instance, one should attempt to discuss the configuration of the other side of the moon or the nature of the inhabitants of Mars. An intermediate class of speculative thought comprises discussions that are based upon a large body of established facts though no two authorities may agree completely in interpreting the facts: in such cases the conclusions drawn are nevertheless useful and are an aid to the advancement of science as they serve to draw attention to points in which our knowledge is weak or to indicate important lines of investigation which have been neglected; as an instance of such speculations, I may cite the much-discussed question of the origin and ancestry of vertebrates, a problem that may be discussed with profit though it may never receive a solution which will command universal assent.

¹ Delivered as the Opening Address in a discussion on the Origin of Life, at a joint meeting of the Botanical and Zoological Sections of the British Association, Dundee, September 10, 1912.

I do not propose to attempt a definition of life, which, in agreement with our President,¹ I regard as a practically impossible task. The problem of the origin of terrestrial life seems to me to admit of being resolved into two distinct questions: first, assuming that the innumerable and immensely varied forms of life now seen on the earth arose by a process of gradual evolution from some original form of living substance or primitive type of living being, to try to form an idea as to what this earliest form of life was like; secondly, when we have reached a conclusion as to the nature of the primordial living creature, to discuss the manner in which this *primum vivens* itself originated and how it got its living and maintained its existence. The first of these problems is one which, in my opinion, can be discussed with profit, though not, I fear, with the hope of drawing conclusions upon which all biologists will be agreed; the second appears to me to be scarcely ripe for discussion, the data being at present altogether too inadequate to permit of our arriving at results of real value. I will confine my introductory remarks to these two questions and consider them in the order indicated.

At the present time, I think I am right in saying, the majority of those occupied with the study of living things regard the *cell* as the vital unit, the primary form of living being. One of our most prominent and valuable zoological textbooks, the *Traité de Zoologie Concrète* of Delage and Hérourard, begins with the sentence "Tout ce que vit n'est que cellules." Living things, considered generally, are regarded by most biologists either as single, individual cells or as built up of many cells; as Delage and Hérourard express it, the cell is the simplest protoplasmic organ which is capable either of living alone or which requires only to be associated with others like itself to form beings capable of independent life. Such statements make it imperative to examine into the meaning and application of the term "cell."

The word "cell," as every one knows, is a term which we owe to the botanists, since it was in plants that the cellular composition of the living body was first discovered. The term "cell" was first applied to the limiting membrane or cell-wall and the fluid or viscid contents were regarded as of secondary importance. Hence the primary meaning of the term "cell" was

¹ See Prof. Schafer's Presidential Address, p. 3.

what the word itself implies in ordinary language, a little box or capsule, a small space enclosed in firm walls. But with increased knowledge it became apparent that the fluid contents were the essential living part of the cell and that the cell-wall was of secondary importance, merely an adaptive product of the contained living substance or protoplasm. Hence the word "cell," as used in biology, underwent a complete change in its connotation and came to have a meaning altogether different from that which the word has in common speech, often very puzzling to those unacquainted with the technicalities of the biological sciences, the cell being defined simply as a small mass or corpuscle of the living substance which might either surround itself with a cell-wall—the product of its own secretive activity—or remain naked and without any protective envelope. With still further advance in knowledge, it was found that in every cell entering into the structure of an ordinary plant or animal there was present at least one body of peculiar properties which was termed the *nucleus*; on account of its universal occurrence, as well as the peculiar relations it was found to bear to the life of the organism, this body soon came to be regarded as an essential component of the cell. Thus we arrive at last at the now generally accepted definition of the cell: a vital unit consisting of an individualised mass of the living substance protoplasm containing at least one nucleus.

We are arrived, then, at this point, that the unit-masses of the living substance of which the bodies of ordinary animals and plants are built up are themselves composed of at least two essential parts. Using the term "protoplasm" for the living substance as a whole, we can assert that the protoplasm of an ordinary cell is differentiated into two distinct components, the cytoplasm or body-protoplasm and the nucleus. Here at once the question arises, is this differentiation of the protoplasm a primary characteristic of the living substance which was exhibited by our hypothetical *primum vivens* or is it a differentiation which was acquired in the course of evolution? If the latter alternative be the true one, is the nucleus or is the cytoplasm the more primitive constituent of the living substance or are they both to be regarded as derivatives of a substance yet more primitive? For my part, I cannot conceive that the earliest living creature could have come into existence as a complete cell, with nucleus and cytoplasm distinct and separate; I am

forced to believe that a condition in which a living body consisted only of one form or type of living matter preceded that in which the body was composed of two or more structural components.

It is, I think I may say, the most generally accepted notion among biologists that the cytoplasmic substance of the cell (to which the term "protoplasm" is often restricted) is to be regarded as the primitive living matter. The earliest forms of life have been supposed to be formless masses of protoplasm, without nuclei, the so-called Monera of Haeckel. From such a condition true cells are supposed to have arisen by individualisation of the indefinite mass and acquisition of a specific form and size, together with the differentiation of a nucleus, which on this view would represent the oldest cell-organ but not an essential or indispensable part of a living body. For my part, I find myself obliged to dissent entirely from any such view. Although a definite nucleus, a body of complex structure and organisation, such as we find in the tissue-cells of animals and plants, is undoubtedly to be regarded as a relatively late product of evolution, I believe, nevertheless, that the nucleus contains the oldest and most primitive elements of the living substance and that the earliest forms of life consisted entirely of the characteristic and essential material of the nucleus. In order to elaborate this view further, I must discuss as briefly as possible the nature and constitution of the nucleus.

In different cells the nucleus is seen to vary almost infinitely in form, structure and composition; but this diversity only brings into greater relief the fact that common to all nuclei is the presence amongst the contents of a peculiar substance termed *chromatin*,¹ which occurs in the form of granules or masses distributed in various ways over the framework of the nucleus. In addition to the chromatin contained within the nucleus, however, there may also be grains of chromatin scattered through the cytoplasm, so-called *chromidia*. In many organisms, finally, a true nucleus may be temporarily absent, the chromatin-substance being present only in the diffused or chromidial condition.

¹ In the course of the discussion Prof. M. Hartog challenged me to give a definition of chromatin; I replied that I would almost as soon attempt to define life itself. I may add that I have discussed the question of the nature of chromatin at greater length in my recently published work, *An Introduction to the Study of the Protozoa* (Arnold, 1912).

The chromatin-substance receives its name from its peculiar property of combining with certain dyes, whereby it can be coloured selectively and differentiated more or less completely from the rest of the protoplasm. The staining test is, however, a very inadequate and untrustworthy method of recognising the substance. Our conception of chromatin should rather be founded upon its relations to the life of the organism as a whole and to its vital activities ; these relations I can only indicate very briefly and summarily, so far as they are known. To begin with, the chromatin-substance is never absent from any known organism, however minute or apparently simple in structure : direct experiment has shown that a cell deprived of its nucleus cannot maintain its life during any length of time and is unable to initiate any of its characteristic vital activities. The reproduction of a cell or of a simple protoplasmic organism always involves division into two or more parts and in this process the chromatin-substance divides first and is partitioned amongst the daughter-individuals. In many cells, the nucleus divides by a very elaborate mechanism known as karyokinesis, which ensures an exact quantitative and qualitative partition of the chromatin between each of the two daughter-nuclei. Throughout the series of living beings, wherever sexual phenomena are observed, the sexual act consists essentially in the union of chromatin from two distinct organisms ; the ascertained facts of fertilisation and development have led to the belief which, if not universal, is at least very widely spread among biologists, that the chromatin-grains determine the characters of the offspring and are the carriers of hereditary tendencies and properties. In the internal economy of the cell, the special function of the nucleus appears to be that of producing the peculiar substances known as ferments or enzymes, substances which perhaps more than any other are characteristic of living bodies.

These data, taken together, in my humble opinion constitute a very strong case for regarding the nuclear substance, chromatin, as the all-important and essential constituent of living organisms. Such a conclusion is greatly strengthened by the fact that some of the minutest forms of life appear to consist entirely or almost entirely of chromatin. Apart from the Chlamydozoa, the true nature of which can hardly be said to be established with certainty at present, many instances could be cited of organisms or stages of organisms in which the body

appears to consist of little or nothing more than chromatin, as for example the spirochætes (treponemes) parasitic in blood, the male gametes of the malarial parasites, etc. It may be urged against this statement that in such minute organisms microscopic technique fails to reveal all details of structure and that cytoplasmic elements may be present though invisible; but at least this much can be said, that the more minute the organism, the less evident, as a rule, is the presence of cytoplasmic structures, until in the very smallest the body appears to consist mainly or even entirely of chromatin.

For these various reasons, I am unable to share the view that the cytoplasmic substance of the cell is to be regarded as the *primum vivens* of which the chromatin and the nucleus are a secondary elaboration.¹ Rightly or wrongly, I have been

¹ The conclusion that the chromatin represents the primary living substance of the protoplasm is one that has been reached by me mainly upon morphological grounds; it stands, therefore, urgently in need of support from other methods of approaching the question and especially from the chemical side. In this connexion, I may quote from a letter of the date August 17, 1912, written to me by a friend whom I know as yet only by correspondence, Dr. R. G. Eccles, of New York, who makes some suggestions which I am not competent to criticise but which seem to me extremely pertinent to the matter under consideration. Dr. Eccles writes:

"If some of your biochemist friends could be induced to present Kossel's ideas of the protamines in connexion with your paper it seems to me it would strengthen your position. The protamines are the proteins most common in spermatozoa. Chittenden refers to Kossel's views thus: 'The basic protamines are undoubtedly the simplest and lowest in the scale and it is quite probable, as suggested by Kossel, that these substances constitute the nuclei of all proteins' (*Pop. Sci. Monthly*, December 1904, p. 157). On the same subject Mann tells us that 'The radicles of which protamines are built up may be as numerous as they are in other albumins but there is less variety and each kind is repeated with great regularity in the different protamines. Kossel believes the protamines to be the simplest albumins' (*Chem. of the Proteids*, p. 420).

"All proteins (albumins) are built out of amino-acids just as houses are built of bricks or stones. There is one amino-acid, arginine, that constitutes 80 per cent. of the protamine, salmine, from the spermatozoa of the salmon. Arginine is the *only* amino-acid found in *all* proteins (albumins) (*Chem. of Proteids*, p. 154). It is the maximum constituent of the proteins of the nucleus and the minimum constituent of the proteins of the cytoplasm. There are three amino-acids—tyrosine, phenylamine and tryptophane—that reach their maximum in the cytoplasm and their minimum in the nucleus. Some protamines seem to contain none. Arginine belongs to the uncomplicated chain-series of carbon compounds (aliphatic), whilst the other three belong to the complex ring-series of carbon compounds (aromatic). The aliphatic chains are the very simplest of carbon compounds. The aromatic rings are complex and are only conceivable, genetically, as arising from the aliphatic chains. It is thus seen that (1) the protein-molecules

led to the conviction that the earliest forms of life were extremely minute ultramicroscopic particles consisting of chromatin alone. I do not lay claim to any novelty in this view; I put it forward simply because I believe it to be true. In the process of gradual evolution and adaptation to divers conditions of existence these minute chromatin-particles formed round themselves envelopes and coats of substance other than chromatin and so gave rise to cytoplasmic elements. As the body was thus increased in size, the next step would be an increase in the number of granules of chromatin contained in it. At this stage the bacterial type of organisation could have arisen by the secretion of a firm membrane at the surface of the body enclosing one or more grains of chromatin (chromidia) in a small amount of cytoplasm. I am far from regarding the bacteria as the earliest or simplest possible forms of life, as some authorities seem to hold; they appear to me rather to represent a type of organisation which arose very early in the evolution of living beings, long before the divergence into animals and plants which dominates modern terrestrial life; a type in which the characteristic limiting membrane has inhibited further advance in evolution and has restricted their structural differentiation within a narrow range. I consider this at least a more feasible interpretation of the nature of the bacteria than the view held by many that they are to be derived from organisms primitively of cellular structure which have become highly specialised for a parasitic or saprophytic mode of life.

The absence, on the other hand, of a rigid membrane or cuticle round the bodies of some of our imagined primitive organisms would permit the formation of a greater amount of cytoplasmic substance and an increase in the number of chromatin-grains in a larger body. Thus would be possible an organism of dimensions relatively large, indeed gigantic as compared with

of the nucleus, that are characteristic of the same, are simpler in construction than the protein-molecules of the cytoplasm and, therefore, most likely more primitive; (2) that the protein-molecules of the cytoplasm, that are characteristic of it, are more complex than those characteristic of the nucleus and less likely to be primitive; (3) that the amino-acid characteristic of the chief nuclear protein is an open chain free from complexity, whilst the amino-acids characteristic of the cytoplasm are closed ring compounds that *could only arise* from chemical compounds of the same type as that characteristic of the nucleus. These facts, I believe, have an important bearing on your subject."

its earliest ancestors, though still microscopic to our limited senses; it would be a mass of cytoplasm containing numerous chromidial grains and it is only in this sense that I can accept Haeckel's *Monera*. The next step in evolution would be the concentration and organisation of the scattered chromidia into a definite compact structure, the nucleus; with this step completed the condition of the true cell would have been reached. Unless the word "cell" is to become quite vague and meaningless, a mere synonym of such terms as *microbe* or *micro-organism*, it should in my opinion be restricted in its application to those organisms which have reached the degree of structural complexity found in the tissue-elements to which the term "cell" was originally applied—that is to say, to organisms in which the protoplasm is differentiated into cytoplasm and nucleus definitely marked off from one another. By this criterion the *Bacteria* and their allies should not be termed cells at all. For me the term "cell" connotes a stage in the evolution of living beings which the *Bacteria* have not reached.

The evolution of the cellular type of structure may be regarded as the most momentous event in the evolution of living beings on this globe. As I have pointed out elsewhere,¹ the cell, in the sense in which I use the word, should be regarded as the starting-point in the evolution of the entire animal and vegetable kingdoms, the elementary structural component of the bodies of ordinary plants and animals. Moreover it is probable that the peculiar phenomena of sex and sexual behaviour did not come into existence until the cellular type of structure had been evolved; in my opinion, without sex there can be no true species in living organisms.²

¹ Presidential Address to the Quekett Microscopical Club, 1911.

² In the course of the discussion, my views with regard to the fundamental importance of chromatin in all living substance were criticised by Prof. MacDonald on the ground that some of the most essential and important activities of the human body were due to purely cytoplasmic structures, as for example all muscular and nervous mechanisms. I am well aware that in the course of evolution of the cell and of its adaptation to various functions, the cytoplasm becomes of great importance and shows an amount of structural differentiation in excess of that exhibited (visibly at least) in the nucleus. It is not necessary to take cells of the human body as examples of this; the ciliate infusoria furnish instances of cells far more complicated in structure than any found in the human body. It seems to me, however, that any attempt to gain a notion of the most primitive type of living being must begin by seeking to discover what, if anything, is common to all forms, moods or shapes of life, rather than by dealing with the complex structures

Having stated my views with regard to the nature of the earliest forms of life, we may now consider briefly the possible origin of the primitive living organism. Here, however, we find ourselves at once on uncertain ground, where the obscurity which the present state of our knowledge cannot penetrate makes it as easy to frame vague hypotheses and speculations as it is difficult to find any solid basis upon which to take a firm stand. Almost all that we can do with any profit is to limit to a certain extent the possibilities of the problem by means of certain propositions, for the most part of a negative order and therefore not a very sure foundation for deductions. Thus from the conclusions of astronomers and physicists with regard to the past history of our solar system, it appears highly probable that the terrestrial globe was once an incandescent mass at a temperature very much higher than that at which life of any kind can exist; consequently there must have been a period at which there could have been no living things on the earth. On the other hand, from all scientific experience it appears to be an established truth, so far as a negative proposition can ever be established, that living things at the present time are produced only as the offspring of pre-existing living things and do not arise *de novo*. From these two propositions taken together, it may be concluded that there must have been a period or epoch of time during which terrestrial life originated. Then there remain two possibilities, the first that life took origin on the earth itself, the second that it was brought to our planet in some way from without.

Biologists, I think, have generally been inclined to favour the first of these views, namely, that terrestrial life was generated on the earth itself; physicists, on the other hand (using the word "physicist" in its widest sense), have been more prone to take the view that living particles were wafted on to our planet from interstellar space. It seems to me that the final word in the matter will lie with the chemists. The main difficulties of the

presented by the most specialised types of living beings. In my opinion there is only one thing common to all forms of life, to the bacterium as well as to man or to the oak-tree, that is the chromatin-substance. Further, I do not think that the evolution of living beings can be considered profitably from the highest forms downwards and backwards; it is best worked, in my opinion, in the direction it must have taken—that is to say, from the simplest and apparently earliest forms of life onwards to the more complex and specialised.

problem are, first, to understand how the complex protein-compounds of which living bodies are constituted could have arisen in Nature; secondly, given the primordial living thing, whatever it was, how it could have maintained its existence on an uninhabited earth—that is to say, what it could have fed on. Both these questions are essentially chemical problems. If living things first originated on the earth, the complex proteins composing the living substance must have been synthesised by some natural process as yet totally unknown; and the same is true *a fortiori* if living matter originated off the earth. A terrestrial synthesis of proteins makes the food-problem somewhat less difficult, since it may be supposed, as Lankester has suggested, that the *primum vivens* supported life on the compounds produced as antecedent stages in its own evolution. On the other hand, it is almost painful to think of a minute living creature wafted from infinite space on to an absolutely barren and sterile earth; the imagination fails to conceive, with such guidance as the present state of knowledge supplies, how it could have got on at all. To obtain light on such questions requires far greater knowledge than we possess at present, not only of the chemistry of the proteins but also of the processes of metabolism and the modes of life of the minuter organisms. It is my conviction that there is a vast field as yet unexplored in this direction and that in the future forms of life will be discovered the very existence of which is as yet unsuspected. Invisible forms of life are now known to exist the discovery of which is due solely to the disturbances caused by them as parasites of ourselves or of other organisms. Is it not then equally possible that other invisible living things exist which, as free-living organisms, produce in their environment effects not as yet perceived by us?

Many theories have been put forward at different times with regard to the origin of terrestrial life.¹ Without attempting to give an account of them in any detail, I may summarise briefly the possibilities that have been suggested. In the first place there is the extreme view, represented by Arrhenius, that life has had no origin in finite time but has existed from all eternity and is coeval with matter and energy—that is to say, that in any period of time to which we can throw our thoughts back, matter, energy and life in some form existed in the universe.

¹ See further my Presidential Address to the Quekett Microscopical Club, 1912

The acceptance of this view puts an end to any speculation on the origin of life; it then simply cannot be discussed at all. On the other hand, the belief is far more prevalent, I think, at least among biologists, that living matter in some form or other arose at some time from that which was not living. In that case there are two further possibilities: first, that life originated on the globe only at some particular period of the earth's history, under special conditions of some kind which do not now exist; secondly, that the conditions under which life is generated exist always and that new life can be produced in the present or future as well as in the past.

If life arose from not-living materials at any time on our planet, its origin is a matter not only for discussion but for investigation and experiment. For even if it arose under conditions not existing now in Nature, there seems to be no reason why such conditions should not be reproduced artificially. On the other hand, it seems much more reasonable to suppose, as pointed out by the President in his address, that the conditions under which life first appeared on the earth were not different from those now existing; consequently, that if life has arisen once *de novo* on the earth, it can do so again at any time, past, present or future.

Why then do we not see new forms of life appearing on the earth? In the first place, I doubt very much if we are acquainted as yet with the simplest forms of life or should be able to recognise them or be aware of their existence at their first appearance. But apart from that, there is another thing to be taken into consideration, as pointed out by Dr. F. J. Allen at the meeting of the British Association in 1896, namely, that if those substances, whatever they may be, which constitute the simplest form of living matter or the transitional stage between the living and the not-living were generated now in Nature, they would almost certainly become the prey or the food of some more highly specialised type of existing living being. From this consideration it follows that the evolution of life on the earth could have had only one starting-point. Just as the dominance on the earth at the present time of an intelligent animal, Man, would prevent the evolution of another animal equally intelligent; so, when once specialised types of living beings had been evolved, a later generation and evolution of life on the earth would have been impossible. In other words,

the origin of the totality of living beings, as known to us, was a historical event which cannot be repeated on the earth unless by some means all existing terrestrial life be destroyed and a fresh start permitted on the *tabula rasa* of the earth's surface.

But even if this conclusion be granted, the origin of life would remain still a subject for investigation and experiment : for if the conditions for generating new life exist in Nature, it is conceivable that they can be reproduced in the laboratory ; and if the only check to renewed generation of living matter in Nature be the existence of specialised forms of living beings, that is a check which could easily be removed in an artificial environment.

It must, however, be pointed out that all these conclusions are purely speculative and hypothetical, resting upon no sure basis of established fact but assuming the occurrence of processes of which, as yet, we know nothing whatever. In the present state of scientific knowledge, our attitude towards the problem of the origin of life must be one of expectancy, of hope for more light in the future. At the point at which we stand it is not possible to frame any hypothesis which can have greater value than that of a pious belief. Whether it will ever be possible to advance beyond this point in our speculations the future alone can show.

THE ORIGIN OF LIFE: A CHEMIST'S FANTASY

"Behold, the beginning of philosophy is the observation of how men contradict each other and the search whence cometh this contradiction and the censure and mistrust of bare opinion. And it is an inquiry into that which seems, whether it rightly seems; and the discovery of a certain rule, even as we have found a balance for weights and a plumb-line for straight and crooked. This is the beginning of philosophy."—EPICTETUS.

THE Presidential Address delivered recently to the British Association at Dundee by Prof. Schäfer and the subsequent independent discussion, at a joint sitting of the Physiological and Zoological Sections of the Association, of the subject considered in the President's discourse will at least have served as a corrective to the wave of vitalism that has passed over society of late years, owing to the pervasive eloquence of Bergson and other writers who have elected to discuss the problems of life, mainly from the metaphysical and psychological points of view, with little reference to the knowledge gained by experimental inquiry.

As Prof. Schäfer himself remarked, the problem of the *Origin of Life* is at root a chemical problem. It is somewhat surprising, therefore, that the chemists were not invited to join in the debate at Dundee: judging from the remarks that fell from several of the speakers, their sobering presence was by no means unnecessary; it is clear that, so long as biologists are satisfied with the modicum of chemistry which is now held to serve their purpose, they will never be able to escape from the region of vague surmise.

On the Tuesday Prof. Macallum fancifully pictured the earth as at one time "a gigantic laboratory where there had been a play of tremendous forces, notably electricity, which might have produced millions of times organisms that survived but a few hours but in which also, by a favourable conjunction of those forces, what we now call life might have come into existence." I think I heard him then refer to the great stores of

oil we now possess and imply that they came into existence in those times. Chemists and geologists would be in agreement, I believe, that these oils were formed at a somewhat late geological epoch and that they are derived from fatty materials laid down as remains of organisms.

Prof. Benjamin Moore, brimming over with biotic energy, afterwards told us that "something more than structure was necessary for life." He preferred a dynamic view which embraced energy, motion and change . . . all the actions of the cell were concerned with the liberation of energy and its transformation into many forms. For the origin of life . . . it was necessary to start with the formation of organic bodies. The colloids, which were large aggregates of molecules, began to show the properties of dawning life but it was needful also to get an energy transformer attached to the colloid. He also insisted that "the problem was metaphysical at the present moment, as through all the ages the process of life was going on. As soon as the colloids got under the influence of sunlight they started synthesising organic bodies. That process was going on now."

In making such statements Prof. Moore allowed his imagination to run away with him; his assertions cannot be justified. Vague, sweeping generalities are out of place in such a discussion. Unless the steps be made clear, there can be no logic in the argument.

No doubt something more than structure is necessary for life. Nevertheless life is dependent on structure—just as is the activity of the steam-engine. The steam-engine is essentially a dynamic machine: it lives only when fuel is burnt under its boiler; but the energy liberated in combustion is brought into action through the agency of a complex mechanism. And it is worth noting that by a slight extension of this mechanism the engine may be made to "remember" and even talk. Thus, if it be caused to draw a steel tape across the magnetic pole of a telephone while the drum of the instrument is being talked at, the message is taken down by the tape; if the tape be then drawn back in the reverse direction, the drum of the telephone will speak and deliver the message remembered in the tape. Surely such an analogy with life is worth considering. Of course it will be said that the engine is fashioned by an intelligence external to itself and if we

suppose that life may have been self-constituted, to obtain a hearing, we must discover the means of self-constitution.

Sir William Tilden, in a letter to *The Times* (September 10, 1912), after referring to the various raw materials available on the earth, remarks: "I venture to think that no chemist will be prepared to suggest a process by which, from the interaction of such materials, anything approaching a substance of the nature of a proteid could be formed or, if by a complex series of changes a compound of this kind were conceivably produced, that it would present the characters of living protoplasm." He appears to deprecate discussion of the problem, judging from the concluding sentence of his letter:

"Far be it from any man of science to affirm that any given set of phenomena is not a fit subject of inquiry and that there is any limit to what may be revealed in answer to systematic and well-directed investigation. In the present instance, however, it appears to me that this is not a field for the chemist nor one in which chemistry is likely to afford any assistance whatever."

I agree with Sir William Tilden that Prof. Schäfer's address "leaves us exactly where we were" and that the "earlier part of the discourse leaves open the question as to a criterion by which living may be distinguished from non-living matter." But I cannot accept his statement that "we have at present, therefore, no clear idea as to what life is and therefore no clear road open to the study of the conditions under which it originated."

Like Prof. Schäfer, I do not find myself in the least helped by the idea that life has originated elsewhere—by adopting such a conclusion we only shift the difficulty a stage further back. I agree too with Prof. Minchin in thinking, that if life had reached us from other worlds it would have found our earth unprepared to receive it and would have been starved out of existence; this question of food supply has not been taken into consideration by the advocates of the hypothesis. If there be life elsewhere, on other worlds than ours, the probability is that it more or less resembles life as we know it. To judge from spectroscopic evidence, the materials of which our world consists are those which constitute the cosmos. There is but one element in which the potency of life can be said to exist—the element carbon; the complexities and variations which are met with in

animate material are only possible apparently in a material of which carbon is the essential constituent. Carbon stands alone among the elements. It is the only one known to us whose atoms hang together in large numbers and can be arranged in a great variety of patterns. The peculiarities of animate matter may certainly be said to be in large measure determined by the presence of carbon, though nitrogen and oxygen, of course, play an all-important part. Our peculiarities may well prove to be traceable ultimately to those of the elements of which we are built—indeed it cannot well be otherwise—yet the difference must be vast between elementary material and living material. It is waste of time, I believe, to pay much attention to the argument from analogy; indeed I feel that Prof. Schäfer relied too much on analogy in the earlier part of his address.

As Dr. Haldane points out—"Living organisms are distinguished from everything else that we at present know by the fact that they maintain and reproduce themselves with their characteristic structure and activities. Nothing resembling this phenomenon is at present known to us in the inorganic world." I do not understand, however, why he goes on to say, "and if, as we may confidently hope, similar phenomena are ultimately found in what we at present call the inorganic world, our present conception of that world as a mere world of matter would be completely altered." Of course it would but the eventuality is one that I, as a chemist, cannot contemplate as possible; far from having confident hope, I believe such discovery to be out of the question.

Prof. Schäfer says the contention is fallacious that growth and reproduction are properties possessed only by living bodies and refers to the growth of crystals—but in this and not a few other cases, as I have said, he carries the argument from analogy too far. The growth of crystals is a process of mere apposition of like simple units, which become assembled, time after time, in similar fashion like so many bricks; and there is no limit to crystal growth; given proper conditions, large crystals inevitably increase at the expense of the smaller similar crystals present along with them in a solution—hence it is that occasionally in Nature crystals are met with of huge size. The multiplication of similar crystals is the consequence of the presence of a multiplicity of nuclei in a solution; nothing corresponding to cell division is ever observed in cases of

inorganic growth. Organic growth is clearly a process of extreme complexity, one that involves the association by a variety of operations of a whole series of diverse units.

It is impossible to regard demonstrations such as Leduc has given with silica and other simple colloids as in any way comparable with the phenomena of organic growth.

Moreover, Loeb's experiments are wrongly quoted by Schäfer as instances of *sexual* reproduction—what Loeb has done has been to show that the life cycle may be started afresh by the introduction of an excitant into the ovum and has thereby shown that the process of fertilisation by the spermatozoon is one in which at least two events are scored—the one being the incorporation of male elements with female elements, whereby biparental inheritance is secured; the other the introduction of an excitant (hormone) which conditions the renewal of the vital cycle of the organism—but the development is that of an incomplete being whose somatic cells lack half the normal number of chromosomes.

Three years ago, in my 'address to Section B of the British Association at Winnipeg, I had the temerity to do what Sir William Tilden says no chemist will be prepared to do—as witness the following passage :

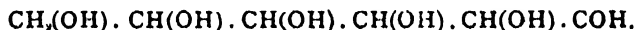
"The general similarity of structure throughout organised creation may well be conditioned primarily by properties inherent in the materials of which all living things are composed—of carbon, of oxygen, of nitrogen, of hydrogen, of phosphorus, of sulphur. At some early period, however, the possibilities became limited and directed processes became the order of the day. From that time onward the chemistry prevailing in organic nature became a far simpler chemistry than that of the laboratory; the possibilities were diminished, the certainties of a definite line of action were increased. How this came about it is impossible to say; mere accident may have led to it. Thus we may assume that some relatively simple asymmetric substance was produced by the fortuitous occurrence of a change under conditions such as obtain in our laboratories and that consequently the enantiomorphous isomeric forms of equal opposite activity were produced in equal amount. We may suppose that a pool containing such material having been dried up dust of molecular fineness was dispersed; such dust falling into other similar pools near the crystallisation point may well have conditioned the separation of only one of the two isomeric forms present in the liquid. A separation having been once effected in this manner, assuming the substance to be one which could influence its own

formation, one form rather than the other might have been produced. An active substance thus generated and selected out might then become the origin of a series of asymmetric syntheses. How the complicated series of changes which constitute life may have arisen we cannot even guess at present; but when we contemplate the inherent simplicity of chemical change and bear in mind that life seems but to depend on the simultaneous occurrence of a series of changes of a somewhat diverse order, it does not appear to be beyond the bounds of possibility to arrive at a broad understanding of the method of life. Nor are we likely to be misled into thinking that we can so arrange the conditions as to control and reproduce it; the series of lucky accidents which seem to be required for arrangements of such complexity to be entered upon is so infinitely great."

It is permissible now, perhaps, to enter somewhat more at length into an explanation of the changes contemplated in this passage.

Growth most certainly proceeds on determined lines—"directive influences are the paramount influences at work in building up living tissues" (Winnipeg address). What Prof. Schäfer has not pointed out, in contrasting the growth of inorganic and of animal matter, is that Nature now works on very narrow lines, making use of but little of the wealth of material primarily at her disposal. Selective influences must have been at work from the earliest stages of the evolution of life onwards. It is in this respect perhaps more than any other that the inorganic differs so greatly from the organic; it is this circumstance too more than any other which makes it so improbable that life should arise frequently *de novo* from simple materials not themselves the products of vital action.

To give an example, the hexose, glucose—a constituent of every plant and animal—is one of sixteen isomeric compounds, all represented by the formula

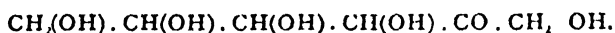


Of these sixteen compounds, fourteen have actually been prepared in the laboratory and they differ considerably in properties. The differences are due to the different distribution in space of the H and OH groups relatively to the carbon atoms. The sixteen compounds form eight pairs and as the individual members of each pair have the power of rotating polarised light in opposite directions, though to an equal

extent, they may be said to be half right-hand and half left-hand material.

Two other hexose sugars isomeric with glucose occur naturally—galactose and mannose ; but the three compounds all belong to the one series and all may be said to be right-hand material.

Besides these three hexose sugars, plants also contain the ketose, fructose, which is isomeric with glucose and differs from it only in containing the CO group as the second instead of as the terminal member in the chain of radicles composing the molecule :



Fructose is convertible into glucose and *vice versa*. Natural fructose and glucose are both right-hand material. Nature apparently is single-handed and can make and wear only right-hand gloves.

It is possible to prepare such compounds in the laboratory from the simplest materials, starting from carbonic acid— $\text{CO}(\text{OH})_2$ —the compound from which the plant derives carbon. By reduction this is first converted into formaldehyde, COH_2 . When digested with weak alkali, this aldehyde is in part converted into fructose ; the fructose that is formed, however, is not merely the form which is found in plants but a mixture of this with an equal proportion of the left-hand form. When the chemist makes gloves, he usually cannot help making them in pairs for both hands.

Some directive influence is clearly at work in the plant—the formaldehyde molecules, which it undoubtedly makes use of as primary building material, in some way become so arranged that when they interact they give only the right-hand form of sugar ; there is reason to think, moreover, that the action takes place only in this one direction—that the sugar is the only product. My own belief is that the synthesis is effected against a sugar template¹ just as a brick arch is built upon a wooden template curved as the arch is to be curved.

A similar argument is applicable to the albuminoid or protein matters derived from animal and vegetable materials ; in fact, to nearly all the natural optically active substances : these are all formed under directive influences. It is not improbable

¹ *Proceedings of the Royal Society*, 1904, vol. 73, 541.

that, excepting a few which presumably are products of retrograde changes, they are all of one type—right-hand material; and apparently they stand in close genetic connexion.

Prof. Minchin has difficulty, he says, in understanding how the complex proteins could have arisen in Nature. But the difficulty in accounting for these is no greater than that involved in accounting for the formation of the sugars. The chief difference between the two classes of compound is that whereas the sugars are composed of like simple units, the albuminoids consist of unlike simple units, chiefly the various amino-acids. The carbohydrate may be compared with a house built of bricks alone, the albuminoids with a house built partly of bricks and partly of stone slabs of various shapes and sizes; the latter form of construction permits of a greater variety of pattern but the same building operations are involved in the use of the two kinds of material: though the constructive units are different, in both cases, the pieces are placed in position and fixed by means of mortar in a similar way.

The directive influences at work and which preside over synthetic operations in the plant and animal cell are undoubtedly the enzymes: these apparently serve as templates and either promote synthesis by dehydration or the reverse change of hydrolysis, according as the degree of concentration is varied.

But how, it will be asked, could action have taken place in times prior to the existence of enzymes? What are enzymes and how did they arise?

The activity of enzymes is comparable with that of acids and alkalis, the former especially, with the exception that enzymes act selectively; but whereas acids will hydrolyse every kind of ethereal compound and are active in proportion to their strength and the concentration of the solution in which they are operative, enzymes will act only on particular compounds: hence their special value as "vital" agents. And the same distinction is to be made with respect to the synthetic activity of the two groups of agents.

At present our knowledge of enzymes is vague: we know little of their structure. At most we can assert that they are colloid materials and that in some way or other they are adaptable to the compounds upon which they act. The picture I form of an enzyme is that of a minute droplet of jelly to which

Is attached a protuberance very closely resembling if not identical with the group to which the enzyme can be affixed. A geometer caterpillar attached by its hind legs to a twig, with body raised so as to bring the mouth against a leaf on the twig, affords a rough analogy, to my thinking, of *the system* within which and within which alone an enzyme is active.

In the beginning of things, carbonic acid was doubtless superabundant and reducing agents were not far to seek: under such conditions formaldehyde may well have been an abundant natural product. The production of fructose sugar, if not of glucose, would be practically a necessary sequence to that of formaldehyde.

But at this early stage, under natural conditions, gloves were always made in pairs, left-hand and right-hand in equal numbers; by chance, somewhere, something happened by which the balance was disturbed: some of the left-hand gloves were destroyed perhaps.

It is well known that if a crystal be placed in a saturated solution of its own substance, the surface molecules will attract like molecules from the solution and the crystal will grow. It is not unlikely that a substance may exercise attraction over molecules which are its own proximate constituents—that glucose, for example, may exercise a preferential attraction over molecules of formaldehyde; if such be the case, glucose may itself serve to influence and promote the formation of glucose from formaldehyde.

Granting such a possibility, if by some accident right-hand molecules preponderated in a solution in which the conditions were favourable to the synthesis of new molecules, the influence of pattern would prevail and a larger proportion of right-hand material would be formed. In course of time the left-hand material would die out and only right-hand material would be present—as in the world to-day. The argument is applicable to compounds generally.

Even the formation of enzymes may be accounted for. Under the influence of acid or alkali, colloid particles may well have entered into association with this or that group. But when once formed fortuitously enzymes probably would become the models or templates upon which new molecules would be formed, much after the manner of the dressmaker's model upon which the dress bodice is fashioned.

But it will be said—"Granted even that simple substances can be formed in such ways, surely it is impossible to account for the production of protoplasm." No doubt, this is difficult, especially as the thing we are asked to account for cannot be defined. I am tempted here again to quote Epictetus:

"Whence then shall we make a beginning? If you will consider this with me, I shall say first that you must attend to the sense of words"

— "So I do not now understand them?"

"You do not."

— "How then do I use them?"

"As the unlettered use written words or as cattle use appearances; for the use is one thing and understanding another. But if you think you understand, then take my word you will and let us try ourselves whether we understand it."

The word protoplasm means so little to most people, so much to a few. It is the convenient cloak of an appalling amount of ignorance—perhaps the scientific equivalent of the "Don't fidget, child," addressed to the too inquiring youngster or the biological paraphrase of the older chemist's catalytic action.

Is protoplasm one or many things? A medium or a substance. In saying that "Living substance or protoplasm takes the form of a colloidal solution. In this solution the colloids are associated with crystalloids which are either free in the solution or attached to the molecules of the colloids," Prof. Schäfer scarcely helps us to a definition. Nor are his later suggestions much more helpful. Speaking of the differential septum by which living substance is usually surrounded, he says: "This film serves the purpose of an osmotic membrane, permitting of exchanges by diffusion between the colloid solution constituting the protoplasm and the circumambient medium in which it lives. Other similar films or membranes occur in the interior of protoplasm."

One thing only is certain—that protoplasm cannot be a solution or anything approaching to a solution in character: diverse structure it must have, structure of infinite delicacy and complexity.

Judging from his reference to the simplicity of nuclear material, it would seem that Prof. Schäfer is prepared to regard protoplasm as by no means very complex. But it is inconceivable that the germ plasm, carrying within itself as

it apparently does all the formative elements of the complete organism, should be simple in structure. It must contain a complete series of interconnected templates from which growth can proceed. I have elsewhere stated that protoplasm may be pictured as made up of a large number of curls, like a judge's wig, all in communication through some centre, connected here and there perhaps also by lateral bonds of union. If such a point of view be accepted, it is possible to account for the occurrence, in some sections, of the complex interchanges which involve work being done upon the substances there brought into interaction, the necessary energy being drawn from some other part of the complex where the interchanges involve a development of energy (Winnipeg Address).

My metaphorical wig as a whole may be taken as representing the racial type—the curls as corresponding to separate characters.

I can imagine so complex a structure being formed by a series of fortuitous accidents in course of time but taking into account the extraordinary fixity of natural types, so well expressed in Tennyson's lines :

So careful of the type she seems,
So careless of the single life,

it seems to me improbable that a like series of accidents should recur. It is on grounds such as these that I cannot accord my sympathy to statements such as Dr. Bastian has made and that I cannot accept the suggestion put forward by Prof. Schäfer that life conceivably is arising *de novo* at the present day, let alone that it is the easy process suggested so light-heartedly by Prof. Moore. Where are the materials? Can we say that they exist anywhere?

It is useless for biologists to live in a higher empyrean of their own and to disregard the minuter details which chemical study alone can unravel: they will never be able to solve the complex problems of life or even to grasp their significance unless they pay more attention to the ways in which building stones are shaped and mortar made and in which edifices are gradually reared from such materials.

I have no desire to take exception to the general trend of Prof. Schäfer's address but I cannot help thinking that he altogether underrates the complexity of vital chemical pro-

cesses; while believing that, as he says, "we may fairly conclude that all changes in living substance are brought about by ordinary chemical and physical forces" and that "at the best, vitalism explains nothing," I am in no way prepared to underrate the difficulties before us in finding satisfactory explanations of the Origin of Life.

I see no reason to suppose that life may be originating *de novo* at the present time nor do I believe that we shall ever succeed in effecting the synthesis of living matter.

With regard to Prof. Moore's statement that all the actions of the cell are concerned with the liberation of energy and its transformation into many forms—there is nothing to show that the forms of energy that are operative during life are in any way peculiar. Energy is inherent in matter: apparently its primary form is that known to us as electrical energy; and inasmuch as Faraday's dictum that chemical affinity and electricity are forms of the same power is incontrovertible, moreover as electricity in its passage through matter is frittered down into heat, the mechanical effects associated with life are easily accounted for. As to the origin of consciousness and of psychical phenomena generally, we know nothing—at most we can assert that we are conscious of consciousness. The effects of consciousness may well be the outcome of simple mechanical displacements of molecules such as take place in the steel tape previously referred to in its passage across a magnetic field varying in intensity. If nervous impulses are conveyed not along continuous tracts but through the agency of interdigitating fibres, a mere alteration in the lengths of these fibres would condition a variation of the impulse; the actual conductivity of a continuous fibre would vary also if chemical changes were to take place within its substance. It is easy to see how chemical changes occurring within a nerve or muscle cell would involve an alteration in the osmotic state, which would necessarily be followed by the influx or efflux of water, according as the alteration involved an increase or diminution of the number of molecules in solution. Oscillatory hydraulic changes of this type may well be at the bottom of both nervous and muscular activity in the organism; in fact, there is every reason to believe that we are but hydraulic engines.

According to Prof. Moore, the colloid shows the properties of dawning life; whatever this may mean, I understand him to say that to make it live, it is necessary to get an energy transformer attached to it. It is surprising how little life there is in those who live, how slowly lessons are learnt. The conditions which determine the transformations of energy were laid down generations ago by Faraday—but are disregarded to the present day. There is little that is mysterious about them; all that is required is a proper arrangement of parts. To give an example, a lump of zinc in diluted sulphuric acid constitutes a binary system brimful of latent energy—of energy awaiting transformation but untransformable so long as the system remains binary; on coupling the conjoined metal and acid by means of a relatively electronegative conductor, however, interaction at once sets in, the metal attacks the acid and the acid the metal and energy is set free—primarily as electricity, secondarily as heat. Nothing can stop the transformation if the ternary system be constituted. Apparently no special energy transformer is required but merely a proper arrangement of parts—given the proper arrangement, action is bound to take place, provided always that the system be one in which there is an overplus of energy.

And here comes the rub. In the case of organisms, not a few changes take place which can only occur if energy be supplied. The assimilation of carbon by plants is a case in point: ordinarily this is effected through the agency of sunlight; but it is clear that in some cases, as in the fermentation of sugar, for example, energy set free in a change taking place in one part of a complex molecule may serve to make up a deficiency preventing the spontaneous occurrence of a change of the reverse order in another part of the molecule. It is an important office of the protoplasmic complex apparently to "negotiate" such exchange or transference of energy.

With reference to Dr. Haldane's statement that we cannot express the observed facts by means of physical and chemical conceptions but must have recourse to the conception of organic unity—I am at a loss in the first place to understand what this conception is, if it be inconsistent with chemical conceptions. I am afraid the vague indeterminate phrases of the philosopher make little appeal to the hard heart of the fact worshipper.

My position is that while we do not attempt to account for that we do not understand or cannot express clearly, all that we do understand is well within our compass to explain; moreover, that our power of understanding is growing every day.

I do not see how Prof. Schäfer and those of us who are with him can be said to have ignored the actual fact of the maintenance in "organic unity" of the numerous physical and chemical processes which we can distinguish within the living body. It is far from being the fact that—"The more detailed and exact our knowledge has become of the marvellous intricacies of structure and function within the living body the more difficult or rather the more completely impossible has any physico-chemical theory of nutrition and reproduction become." Or that "the difficulty stands out in its fullest prominence in connexion with the phenomena of reproduction and heredity."

To make my meaning clear, let me go back to my wig. Assuming the primordial wig to have come into existence through a series of lucky, fortuitous accidents, assisted by certain peculiarities inherent in the primary material and favoured by the special conditions of the environment—wigs have ever since been made much on the pattern of the first wig though variations have taken place from time to time.

Each new wig is constructed on top of an old wig and when a new wig is ready, "division" takes place and the new wig is removed to a new "cell" together with a supply of tools and materials required for wig-making. According to the material available, while the general pattern is maintained intact, variations may be introduced into individual curls. But two kinds of wigs are to be thought of: simple wigs—male and female—and compound wigs, the latter being made by superposing two simple wigs after such alterations have been made in each as to permit of their superposition: obviously, when the compound wigs are separated and worn as simple wigs, the new simple wigs differ somewhat from the old though they are very like them in general character; also it will be clear that all sorts of combinations of simple wigs may be made.

Obviously my metaphorical wigs correspond to nuclei and the tools and materials used in making them to the cytoplasmic elements—assuming that the nucleus is the formative element of the cell. Having thus put wigs on the green, I trust that I have met the challenge given by Dr. Haldane and that it will

be obvious that even the problems of reproduction and heredity, if not those of immunity, may be dealt with from some such point of view as that I have ventured to state.

The assertion has been made¹ recently that the scientific world "is beginning on all sides to admit the necessity for postulating the co-operation of some 'outside' factor. Lodge in England, Bergson in France and Driesch in Germany are the most conspicuous apostles of the new movement."

This is but one of the many such statements made of late. An apostle after all is but a messenger and the character of a message depends a good deal on the instruction the messenger has received, though imagination may contribute a good deal to its ultimate adornment. The messages delivered to the public on such a subject are apt to be somewhat imaginary. It is clear that they cannot be even an approximation to truth, when no notice is taken by those who convey them of the results achieved by the toiling workers in the distant adits of the mine of science. Philosophers must go to school and study in the purlieus of experimental science, if they desire to speak with authority on these matters.

Here again I am served by the old Greek cynic—"The beginning of philosophy, at least with those who lay hold of it as they ought, is the consciousness of their own feebleness and incapacity in respect of necessary things." Such sayings make us wonder at the lack of appreciation displayed by the Sage of Chelsea in making Sartor say: "The 'Enchiridion of Epictetus' I had ever with me, often as my sole companion, and regret to mention that the nourishment it yielded was trifling." But he too was a philosopher.

After telling us that the cell is now defined as a vital unit consisting of an individual mass of the living substance protoplasm containing at least one nucleus; and that the protoplasm of an ordinary cell is differentiated into two distinct components—the cytoplasm or body-plasm and the nucleus—Prof. Minchin raises the question whether the cytoplasm or the nucleus is to be regarded as the more primitive. He cannot conceive, he says, that the earliest living creature could have come into existence as a complex cell, with nucleus and cytoplasm distinct and separate; and he is forced to believe that

¹ "Involution": by Lord Ernest Hamilton.

a condition in which a living body consisted only of one form or type of living matter preceded that in which the body consisted of two or more structural components.

The issue thus raised is an important one. Regarding the cell as the vital unit, as "the simplest protoplasmic organ which is capable of living alone," in other words, capable of growing and of reproducing itself, the question I venture to put is whether life did not begin only when the cell was first constituted, whether the materials formed prior to this period, however complex, were not all incoordinated and therefore inanimate.

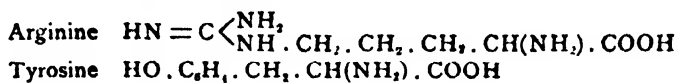
The term cell unfortunately has had somewhat different meanings attached to it. At first, as Prof. Minchin tells us, only the limiting membrane or cell wall was thought of, the fluid or viscous contents being regarded as of secondary importance; the primary meaning, in fact, was that of a little box or capsule. It then became apparent that the fluid contents were the essential living part, the cell wall merely an adaptive product of the contained living substance or protoplasm. Consequently, the cell was defined as a small mass or corpuscle of the living substance, which might either surround itself with a cell wall or remain naked and without any protective envelope. Further advance involved the recognition of a nucleus as an essential component of the cell.

I cannot think of a naked mass of protoplasm, call it chromatin (stainable substance) or what you will, playing the part of an organism; at most, I imagine, it would function as yeast zymase functions.

If it is to grow and be reproduced, the nuclear material must be shut up along with the appropriate food materials and such constructive appliances as are required to bring about the association of the various elements entering into the structure of the organism. The enclosure of the naked protoplasmic mass within a differential septum (cell wall) through which only the simpler food materials could gain an entry seems to me therefore a necessary act in the evolution of life. From this point of view, it matters little which came first—chromatin or cytoplasm.

The argument put forward by Mr. Eccles in support of the contention that nuclear material is the more primitive, based on the preponderance of the open chain derivative arginine in the nucleus and of benzenoid derivatives such as tyrosine in the cytoplasm, cannot be regarded as valid. The difference between

open and closed chain compounds is not such that chemists can regard one as more primitive than the other, except it be that the open is the first to receive attention in the text books; and arginine, if not the most, is one of the most complex products hitherto separated from albuminoid materials, far more so than tyrosine:



Arginine probably owes its value as a nuclear material to the many points of attachment its nitrogen atoms offer—in other words, to its complexity.

Professor Minchin would restrict the term cell to organisms in which the protoplasm is differentiated into cytoplasm and nucleus definitely marked off from one another and would therefore deny the term cell to Bacteria and their allies. But Bacteria apparently consist of materials differing but little in complexity from those met with in higher organisms and they contain a variety of enzymes. The separation of the nucleus within a special differential septum would appear merely to mark it off as a separate factory within which special operations can be carried on apart from those effected in the cytoplasm; the extrusion of nucleoli from the nucleus during the vegetative stage is particularly significant from this point of view, especially as the nucleoli within and without the nucleus stain differently.¹ The differentiation of the nucleus therefore may be merely a mark of a higher stage of organisation but to make the distinction suggested between Bacteria and other forms appears to me to be unjustifiable.

From the point of view I am advocating, every organism must possess some kind of nucleus—visible or invisible: some formative centre around which the various templates assemble that are active in directing the growth of the organism. The cell, in other words, is the unit factory and its definition should be made independent of microscopic appearances.

To conclude. All speculation as to the Origin of Life must savour of the academic; it can have no very definite outcome unless it be verified experimentally and at present it seems

¹ See especially "Observations on the history and possible function of the nucleoli in the vegetative cells of various animals and plants." By C. E. Walker and Frances M. Tozer, *Quart. Journ. Exp. Physiol.* 1909, 2, 187.

improbable that such verification will be possible. But speculation is none the less legitimate and desirable on account of the fundamental issues to be considered.

In discussing the problems of heredity, in dealing with disease, we are groping in the dark so long as we are ignorant of the precise nature of the vital processes and of the minute details of organic structure; no effort should be spared therefore to unravel these. The results of modern cytological inquiry are very marvellous but unsatisfactory. We need to know far more of living material, especially in the vegetative stage; the chemist has difficulty in accepting the findings of the morphologist at their face value, he cannot avoid the feeling that not a few of the "structures" described may be artefacts bearing but a distant resemblance to the living forms, as structure is usually brought into evidence by staining and this cannot take place until the differential septa of cells are broken down and rendered permeable; so that the staining and fixing process is one that must be attended with chemical changes, among which coagulation effects are to be reckoned. But the appearances in many cases are too definite, too wonderful, to be mere artefacts.

What is now needed is the combination of the eyes of the cytologist with those of the chemist and with those of the physiologist, the collaboration of the student of external structure and the student of function. Continued specialisation can only carry us further away from the goal we are all striving at, though vaguely—because we have no settled combined scheme of action.

H. E. A.

THE RESCUE OF FARADAY'S ELECTRO-CHEMICAL RESEARCHES

SOME enthusiastic believers in the soul-saving power of education and in the possibility of imparting school-learning to the masses generally may have dreamt of bringing science to the doors of the public at large but it has remained for Messrs. Dent & Son to make the actual experiment.

Instead of inviting some more or less obscure individual to write a cheap, trashy text-book, with commendable foresight they have republished, as one of the volumes in their well-known *Everyman's Library* series, the whole of Faraday's wonderful electrochemical researches communicated to the Royal Society of London in the years 1833-4 and 1840—that is to say, Nos. III to VIII, XVI and XVII, in which the foundations of electrochemical science were first laid down. The reprint is from the issue in three volumes of Faraday's papers published in 1839-55, in which foot-notes were added to the original papers; unfortunately the paragraphs have been renumbered and dates are not attached.

Messrs. Dent & Son have rendered an invaluable service to the cause of scientific education. It is to be hoped their venture will meet with the recognition and success it deserves.

No happier choice could possibly have been made. Black's short essay on *Magnesia Alba* (*Alembic Club Reprints*, No. 1) and these early memoirs of Faraday are the most conspicuous examples of true scientific method it is possible to put before the student—it is safe to say that the two books, costing together half a crown, are worth all the elementary text-books on chemistry put together that are in use at the present day in school or college.

We would counsel every serious student of science to possess the volume—to study it line by line, paragraph by paragraph, if only as a model of literary style and as an example of clear, incisive, logical and purposeful writing. Whoever learns to appreciate the lessons of truth that are conveyed in

its pages should be fairly proof against the scientific immorality characteristic of our time. Faraday's transparent honesty of purpose, his marvellous gift of insight, his wonderfully philosophical mind afford a striking contrast to the dogmatism and narrowness of outlook which have prevailed of late years, especially in the field which he was the first to cultivate: unfortunately the details of his work have long been buried in oblivion and the lessons to be learnt from him are in no proper way brought home to the student.

Those who propose to study the memoirs should prepare themselves by reading a life of the author. The introduction by which the volume is prefaced is not one specially written for the occasion but is taken from Tyndall's *Faraday as a Discoverer* and is scarcely suitable. It is essential to know something of the man to understand his work, to appreciate his wonderful performances. His origin, the manner of his introduction to the Royal Institution, the extraordinary way in which he trained himself both as chemist and physicist, before all things his character must all be considered in connexion with his achievements.

The perfection of his literary style is altogether marvellous. This is particularly noticeable in the first memoir in the book—that dealing with the identity of electricities derived from different sources. The simplicity and directness of the questions put and at once tested experimentally, the swiftness and sureness of the attack, the transparent honesty of purpose maintained throughout the work are wonderful enough, taking into account the state of knowledge at the time and Faraday's previous experience; but the purity of diction and the lucid and logical manner in which the work is described and the argument developed are even more noteworthy. Polite letter-writers have served their purpose in the past: if those who aim at accomplishing scientific work take these memoirs of Faraday as their model, far fewer complaints will be made in future of the style of authors of papers on scientific subjects.

It is only necessary to call attention to a few of the plums in the book. The memoir "On the power of metals and other solids to induce the combination of gaseous bodies" is one that should be studied by all who are interested in "catalytic" phenomena. Little has been added to our knowledge of the subject which is not either contained or foreshadowed in this essay.

The researches which led to the establishment of Faraday's law, of course, are classic. The following statement, made in paragraphs 254, 255 and 260, embodies practically all that can be said even now, with any degree of conviction, of our knowledge of the process of electrolysis :

254. "Passing to the consideration of electrochemical decomposition, it appears to me that the effect is produced by an *internal corpuscular action* exerted according to the direction of the electric current and that it is due to a force *either superadded to or giving direction to the ordinary chemical affinity* of the bodies present. The body under decomposition may be considered as a mass of acting particles, all those which are included in the course of the electric current contributing to the final effect; and it is because the ordinary chemical affinity is relieved, weakened or partly neutralised by the influence of the electric current in one direction parallel to the course of the latter and strengthened or added to in the opposite direction, that the combining particles have a tendency to pass in opposite courses."

255. "In this view the effect is considered as essentially dependent upon the mutual chemical affinity of the particles of opposite kinds. . . ."

260. "I suppose that the effects are due to a modification, by the electric current, of the chemical affinity of the particles through or by which that current is passing, giving them the power of acting more forcibly in one direction than in another and consequently making them travel, by a series of successive decompositions and recompositions, in opposite directions and finally causing their expulsion or exclusion at the boundaries of the body under decomposition, in the direction of the current. . . ."

The discussion "On the source of power in the voltaic pile," in which the contact hypothesis is discarded by Faraday, is one that deserves renewed attention at the present time. Owing to the fact that Lord Kelvin's great influence was exerted in favour of direct contact action, the explanation has regained favour—but it is very doubtful whether the arguments that have been put forward in support of this view are valid : at least they require reconsideration. But physicists are now so much concerned with metaphysics that fundamental problems in electrochemistry appear no longer to interest them. The publication of Faraday's early memoirs may serve, in some measure, to redeem the situation; sometimes in set words but always implicitly he was an advocate of the doctrine that truth is the one possible foundation of science.

STARCH: A CAPITAL DISCOVERY

It has long been established that starch, the first *visible* product of the assimilation of carbon dioxide by plants, is resolved by the enzymes known collectively as *diastase* into a mixture of so-called dextrans and maltose, the isomeride of saccharose or cane sugar; acids have a similar effect but by their action the starch is ultimately reduced to glucose. Starch is represented empirically by the formula $C_6H_{10}O_5$ but it must be supposed that a considerable number of such units are present in its molecule, each derived from a molecule of glucose. The dextrans apparently are all intermediate in complexity between starch and maltose; they are ill-characterised substances and with one exception have been described as non-crystalline.

Needless to say, knowledge of the structure of starch is of primary importance but chemists hitherto have met with little success in their attempts to determine the manner in which the C_6 units are associated. At last, however, light is coming and again we are helped by the humble *Bacillus*. It was pointed out by F. Schardinger, in 1903, that crystalline products might be obtained from starch by the action of certain Bacteria. Schardinger then isolated the active organism (*Bacillus macerans*) and with its aid succeeded in obtaining an α - and a β -dextrin, which he described somewhat fully.¹

Messrs. H. Pringsheim and H. Langhans, who have undertaken the further study of these compounds, have arrived recently at results of a striking character.²

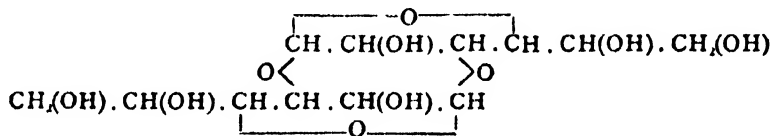
Schardinger's α -dextrin dissolves in water to the extent of 17.9 and the β -dextrin of 1.76 per cent. at the laboratory temperature. The β -compound is at least of the complexity indicated by the formula $(C_6H_{10}O_5)_n$; cryoscopic determinations show that

¹ F. Schardinger, *Zeitschr. f. d. Untersuch. d. Nahrungs- u. Genussmittel*, 6, 874 (1903). *Wiener klinische Wochenschrift* (1904) Nr. 8. *Zentralbl. f. Bakteriologie und Parasitenkunde*, II. Abt. 14, 772 (1905); 19, 161 (1907); 22, 98 (1909); 29, 188 (1911).

² *Berichte d. deut. chem. Ges.* 1912, 2533.

the α -compound has the formula $(C_6H_{10}O_5)_n$. When digested with acetic anhydride and zinc chloride, the two dextrans are not only acetylated but both apparently yield derivatives of half the original molecular complexity, the hexasaccharide giving a non-acetylated trisaccharide and the tetrasaccharide a hexacetylated disaccharide. The corresponding "saccharides," *triamylose* and *diamylose*, are obtained on displacing the acetyl groups by hydrogen: both are crystalline.

Taking into account the composition of the C_6 units—the presence in each of only three hydroxyl groups and of OH_2 less than in glucose—and the fact that both compounds are without action on Fehling's solution and do not exhibit the phenomena known as mutarotation, the following formula is a not improbable representation of the disaccharide:



It may be hoped that at no distant date, with the aid of data such as the discovery under consideration affords and conceptions such as those introduced by Barlow and Pope, it will be possible to arrive at a clear representation of the manner in which the atoms are close-packed even in so complex a molecule as that of starch.

THE MECHANISM OF INFECTION IN TUBERCULOSIS

By R. R. ARMSTRONG, B.A., M.B., B.C. (CANTAB.), M.R.C.S., L.R.C.P.

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"Si l'on veut bien réfléchir, d'une part, aux différences de composition chimique, même qualitatives, qui peuvent exister entre deux espèces très voisines, d'autre part, à l'extraordinaire variété de matières albuminoïdes qu'il est aujourd'hui possible de concevoir, il ne paraîtra pas excessif d'assimiler des espèces animales ou des variétés physiologiques d'une même espèce animale à des milieux de culture variés, analogues à ceux qui m'ont servi pour l'étude de la bactérie du sorbose, ni d'expliquer l'immunité des unes et la réceptivité des autres à l'égard d'un microbe déterminé par une différence chimique ou seulement stéréochimique de leurs parties constituantes.

"Y a-t-il rien de plus suggestif à ce point de vue que ces deux bouillons de levure formés des mêmes matières organiques de toutes sortes, des mêmes bases métalliques, des mêmes acides, mais dont l'un renferme, en outre, de la sorbite et l'autre de la dulcité? Quand on y sème la bactérie du sorbose, le premier est rapidement envahi; la sorbite fait place à un corps nouveau, doué d'une grande activité fonctionnelle; les autres substances disparaissent en même temps, entraînées dans la nutrition du microbe. Le second, au contraire, résiste à l'infection; la bactérie, d'abord languissante, finit par y mourir sans transformer ni la dulcité qu'on retrouve tout entière, ni, pour ainsi dire, aucune des autres substances qui l'accompagnent.

"Il doit y avoir le plus qu'une image, peut-être un enseignement, dont il faudra tenir compte dans la lutte contre certaines maladies.

"Après la belle découverte du sérum antidiphthérique, on avait pu croire terminée l'ère néfaste des microbes pathogènes, espérer l'arrêt définitif des ravages exercés par la tuberculose, le cancer, etc. Il n'y avait plus, semblait-il, qu'à préparer, à l'exemple de Behring et de Roux, un sérum contre chacun des germes morbides. Malheureusement, il a déjà fallu revenir beaucoup sur ces espérances.

"Toutes les maladies, comme toutes les cultures, ne se ressemblent pas. Dépendant les unes et les autres de deux facteurs essentiels: la semence et le terrain, elles sont subordonnées aux conditions de rencontre et de convenance de ces deux facteurs.

"Si la maladie est, comme la culture spontanée d'un végétal parasite dans un liquide ou un terrain vierge, une espèce d'accident dont la fréquence est limitée par la rareté du germe, il devient possible, en faisant disparaître l'organisme étranger qui a pris naissance, de revenir à l'état normal et la propreté du terrain. C'est ce qu'on peut faire à l'aide des sérums dans le cas de la diphtérie ou de la peste.

"Mais si, au contraire, le mal tient au développement d'un parasite dont le germe est si extraordinairement répandu qu'il est impossible d'y soustraire le terrain de culture, ne vaut-il pas mieux, au lieu de l'extirper sans cesse, agir sur le terrain même et le rendre impropre au développement du parasite ?

"Envisagée sous cette forme, la lutte contre la tuberculose trouverait peut-être à gagner. La médecine reconnaît déjà chez les individus arthritiques des circonstances défavorables à l'évolution du bacille de Koch. N'est-ce pas le moment d'étudier quelles sont ces circonstances, de chercher s'il n'y a pas là, au fond, quelque cause d'ordre chimique, quelque chose qui rappelle l'un des bouillons de tout à l'heure vis-à-vis de la bactérie du sorbose ?"—G. BERTRAND (1906).

THE MECHANISM OF INFECTION IN TUBERCULOSIS

A NOTABLE extension of the Public Health Act of 1875 made in May 1911 renders the notification of pulmonary tuberculosis, when diagnosed in hospital in-patients, a statutory obligation. By an extension of this Order the notification of all cases of pulmonary tuberculosis whenever and wherever diagnosed is now recommended.

The Insurance Act, 1912, with the terms of which most people are tolerably familiar, makes provision for the treatment of such cases in hospitals and goes so far as to provide for the actual isolation of persons suffering from pulmonary tuberculosis.

It is admitted by most observers that tuberculous infection, at least in adults, is the result mainly of invasion by way of inspired air of the respiratory tract; but it is equally well known that the tubercle bacillus may gain entry by other means. Milk contaminated with bacilli from tuberculous cows and the flesh of animals which show evidence of tuberculosis are considered to be potent sources of infection.

It is possible, however, that infection through the agency of milk occupies a more prominent position in the public mind than the frequency of its incidence warrants. So much, in fact, has been written on this subject in the daily papers, so many laws and bye-laws are in force to render the lives of unfortunate cow-keepers, dairymen and milk-vendors burdensome, that were it not that individual experience teaches each and all of us that the danger is vastly over-rated, no sane person to-day would ever drink raw milk.

Tuberculosis is no uncommon cause of death in young children. As cows' milk is the staple food of the majority of infants during the first year of life, a period when the relative death-rate is

high, by a somewhat questionable process of reasoning some authorities have placed the blame upon the milk supply.

Inasmuch as our ideas are inevitably the result of impressions received, the result of this insistence has been that not only the majority of the thinking community but many physicians and even the Local Government Board authorities themselves are persuaded that at least in the case of young children milk is the prime source of tuberculous infection.

Far more stringent regulations than those applied to milk control the sale of meat for human food. Beasts showing any evidence of tuberculosis and carcasses in which tuberculous lesions are present are rigorously condemned by the meat inspectors.

Milk being entirely derived from bovine sources in our country and meat very largely so, investigators directed their attention at an early date to such differences as exist between tubercle bacilli occurring in the ox and in man.

DISTRIBUTION OF TUBERCULOSIS

No disease is more universally prevalent throughout the animal kingdom than tuberculosis. A similar disease occurs in reptiles; birds also, especially in captivity, are prone to tuberculous invasion and the avian bacillus has been found to be possessed of definite characteristics; in fact, all animals are susceptible to tuberculosis and in the collection of the Zoological Society, as well as in similar menageries all over Europe, no disease is more prevalent nor more fatal. Bovines seem specially susceptible to pulmonary and abdominal tuberculosis and sometimes suffer from tuberculous disease of the udder.

Of the smaller mammals—mice, rats, guinea pigs, rabbits, etc.—used in laboratories for inoculation experiments, the guinea pig is very readily infected by tuberculous material.

It is scarcely necessary to refer to the great variation in relative susceptibility to tuberculosis which is noticeable in the different races of man. Europeans show a high degree of natural and acquired immunity, whilst the liability to infection of races which have no previous experience of the disease is common knowledge. Thus the North American Indians are said to have been decimated, indeed almost exterminated, by tuberculosis; the Sandwich Islanders afford another illustration

of the disastrous activity of the disease when introduced amongst peoples not previously exposed to its attacks.

On the other hand, the Jewish race is now possessed of a high degree of natural immunity. Though it cannot be said that tuberculosis does not attack Jews, the experience gained in Out-patient Departments of Hospitals for Diseases of the Chest affords unquestionable proof of the fact that pulmonary tuberculosis is of only occasional occurrence amongst them. When met with in this race, moreover, the disease is rarely fatal but assumes a chronic, *i.e.* a mild and slowly progressive form.

The statistics at my disposal of postmortems at the Children's Hospital, Great Ormond Street, demonstrate equally clearly the relative immunity from tuberculosis of the children of Jewish parents. It is possible that the explanation of this remarkable condition lies in the essentially urban character of the Jewish race. Since the Fall of Jerusalem, at latest, the Jews have lived in cities. Through the Middle Ages and during the last century they have successfully encountered persecution and squalor in the poorest quarters of the cities of Europe and have flourished despite the slenderness of the resources permitted them by the ruling race, owing to their thrift and their aptitude as traders, in circumstances under which the less careful indigenous population has gradually succumbed. As a matter of fact, whilst tuberculosis is always rife amongst slum populations, the Jewish members remain practically immune. In fine, one is tempted to hazard the suggestion that by a rigorous process of natural selection a race has gradually been evolved possessing so high a degree of resistance to tuberculosis that at the present day their freedom from this disease justifies the statement that they are naturally immune.

How far breast-feeding is responsible for the vigour of the majority of Jewish children is open to discussion; at all events, their example is cited as a point in favour of cows' milk being the cause of tuberculosis, breast-feeding being far less frequent or prolonged amongst other races, notably the English.

Enough has been said to make clear the wide distribution of tuberculosis in the animal kingdom and the extreme variation in susceptibility to this disease shown by races subjected during longer or shorter periods to its attacks, as well as the relatively high mortality from tuberculosis in early life.

THE MECHANISM OF INFECTION

Careful consideration of the mechanism of infection is clearly of the first importance in view of the wide prevalence and heavy mortality from tuberculosis. When such foods as milk and meat are implicated as causes of the disease and not only the health of the community but vast commercial interests are at stake, the propriety of reviewing the evidence that milk and meat are the carriers of infection is beyond question.

The matter may well be approached by considering the lesions of tuberculosis, as they occur in man at the various stages of his existence, under the varying conditions of function and environment which attend infancy, adolescence, maturity and old age.

Few generalisations are more remarkable than the freedom of infants from disease other than nutritional disorders during the first year of life. Marked as this immunity is in breast-fed infants, it is equally striking in those brought up by hand.

The suggestion has been advanced that by means of the intimate apposition of foetal and maternal blood which attends intra-uterine life, the growing embryo obtains from its mother substances which serve to protect it from the attacks of pathogenic micro-organisms in the early months of its existence. These substances, it may be, are of the nature of anti-bodies, which are preformed by the mother in response to infections which she from time to time incurs; or, perhaps, during the nine months of its foetal existence, the child obtains doses of the commonly occurring bacterial poisons by diffusion from the maternal blood.

In response to the stimulation of these toxins, the child prepares its own anti-bodies, the mother having, in the nature of the case, previously tempered the virulence of the infection below the minimum harmful dose by the exercise of her own protective mechanisms. Even *in utero*, cases are recorded of foetal infection associated with advanced maternal tuberculosis but these are extremely rare.

I would take this opportunity, however, of insisting that so far as the acute infectious diseases of childhood are concerned, milk is not a source of infection in infants, since in children up to the age of one year they scarcely if ever occur. Furthermore the epidemic diseases carried by milk are few. The infection of

scarlet fever is frequently conveyed by milk; that of diphtheria sometimes; that of measles (*Morbilli*) perhaps occasionally and of German measles (*Röteln*) seldom. It is open to question if chicken-pox can be conveyed by milk. Typhoid, it is well known, is not infrequently attributable to the contamination of milk with infected water.

Diphtheria is by no means uncommon in babies but more frequently occurs as a nasal discharge than in its dangerous membranous form; in the nasal form of the disease, bacilli are detected on bacteriological examination but there are no symptoms other than the discharge. Tuberculosis affects children of this age far less commonly than later in life and in cases which I have seen there has commonly been an obvious source of infection in a mother or attendant suffering from pulmonary tuberculosis, often in an active form.

By far the commonest form of tubercle in children is that involving infection of the glands of the neck; but if postmortem evidence be a fair guide, tubercle in the bifurcation gland—the gland situated just below the bifurcation of the trachea into the two main bronchi—is more common still, there being no invasion of glands in the neck in many cases. Frequently the bifurcation gland is the oldest focus found; more frequently still the gland is caseous, *i.e.* shows degeneration as the result of the prolonged activity of tubercle bacilli.

Next in order may be placed tubercle in the abdominal glands, particularly the mesenteric glands—the glands which lie in the goffered, fan-shaped membrane by which the small intestine is attached to the hinder body-wall—which are the first to receive the lymph flowing from the gut.

Tubercle of the peritoneum is not uncommon.

Tubercle occurs frequently in bones—caries of the spine being an outstanding example.

Tubercle of joints, particularly the knee and hip, is common but it is to be noted that tubercle of the joints really begins not in the joint but in the part of the bone which is growing most actively, namely the epiphyseal end—either the upper or lower—of the shaft of the bone; it spreads thence to the joint.

A suggestive hypothesis bearing on the incidence of bone lesions in children is that these parts are, as it were, so busily occupied with the work of growth and so highly specialised in this connexion that they possess but little power of resisting

the attacks of pathogenic micro-organisms. Witness also the extreme susceptibility of the periosteum of the thigh and shin bones to acute streptococcal infections. Furthermore, the ends of the long bones are richly supplied with blood moving through wide spaces in a comparatively stagnant stream ; bacilli reaching the bone by this route are pre-eminently liable to lodge amongst the intricacies of the bony lattice which is being built into the growing bone. These parts too in children are subject to direct injury and this is true of the lower limbs to a greater extent than the upper, a fact in correspondence with the more frequent incidence of tuberculosis at the hip and knee.

There is no more remarkable nor uniformly fatal form of tuberculosis in children than acute miliary tuberculosis. In this disease, tubercle bacilli reach the blood stream in large numbers and being carried to all parts of the body give rise to tiny tuberculous foci scattered through every organ, hence the term miliary—like millet seed. When the brain is also affected, as is usually the case, we have the condition known as Acute Hydrocephalus or "Water on the Brain"—called technically Tuberculous Meningitis.

Much may be learnt from careful consideration of the exact distribution of the tuberculous lesions in these cases and such investigation is of special value, seeing that death within three weeks, seldom longer, is their invariable consequence.

The determining factor in the invasion of the blood by tubercle bacilli and its dissemination in the vital organs remains a matter for conjecture. In no way is it correlated with the number or extent of pre-existing lesions.

It has been my experience in making postmortem examinations of cases of tuberculous meningitis that but one lymphatic gland or group of glands has shown evidence of tuberculosis. Occasionally this gland is to be found in the mesentery, in which case it may be inferred that tubercle bacilli effected an entrance through the gut : most frequently it occurs in the bifurcation gland described above, which is often the only seat of tuberculous infection.

The following case may be quoted in some detail in illustration:

A baby aged eleven months, which had always been fed at its mother's breast, was brought to hospital with the story that it had sustained a fall on its head fourteen days before admission but neither at the time nor during the next few days

seemed any the worse. About five days before bringing it to the hospital, the mother had noted that though the child took the breast as vigorously as ever, it vomited suddenly very soon afterwards; this sickness continued and the child became very fretful, especially when disturbed; later on it was drowsy. She remarked that it was unusually constipated. The day before the child was brought to hospital it was attacked by severe convulsions: soon afterwards it appeared not to recognise the mother and seemed unable to swallow its food. She herself was in good health but her husband was suffering from phthisis, A diagnosis of tuberculous meningitis was made. Despite treatment, the infant became steadily worse and died a few days later. At the postmortem, advanced tuberculous degeneration (caseation) was found in the bifurcation gland. There were large numbers of very recent, tiny miliary tuberculous foci present in the lungs, brain and spleen corresponding in their numerical incidence to the order given above. A few tubercles were to be seen in the liver, very few in the kidneys, none at all in the supra-renal nor the pancreas. In the lower part of the small intestine a few very recent tiny tuberculous ulcers were found. The heart showed no evidence of tuberculosis; the stomach also was unaffected.

The evidence that miliary tuberculosis is spread by the blood stream and that the tubercle bacilli do settle in blood vessels is so strong that for the purposes of this article it may be accepted as proven. Accepting this hypothesis, it is interesting to review the course of events from the time when the infant first sustained an attack from tubercle bacilli till its death.

We may safely suppose that under the social conditions in which persons of the hospital patient class live, the husband shares their couch at night with wife and child. In such intimate contact, the sleeping babe would inhale tubercle bacilli, as his father lay wakeful and coughing through the night. It is my belief, founded on considerations that I shall presently advance, that tubercle bacilli so inhaled do not, in children at least, lodge in the upper air passages in sufficient numbers to give rise to lesions there but that they pass down the windpipe and are carried along with the strong inspiratory air blast directly into the ultimate air-cells of the lungs. Such is the known course taken by bacilli or dust particles when injected into the trachea under experimental conditions.

So relatively innocuous are the bacilli of tuberculosis and so slight is the local disturbance to which they give rise that they are not held up in the lung, as are the virulent organisms which cause pneumonia. Becoming ingested by the cells which line the air-chambers (alveoli) they are passed on into the lymphatics of the lung and find their way to the glands lying at the lung root, which act as filters for the lymphatic system. There apparently they remain, either to be destroyed or perhaps, as in the case under notice, to give rise to slow degenerative changes in the gland substance; the result is the formation of that curious cheese-like or "caseous" material which is so characteristic of tuberculous lesions.

The process of infection continues but the lymph-gland filters prevent bacilli coming from the lungs from entering the blood stream. As time goes on and the gland substance is destroyed, bacilli either periodically find their way directly into the blood coursing through the glands or pass perhaps by way of the efferent lymphatic channels to the main lymph ducts and thus reach the blood. The main lymph ducts in such cases as this do occasionally show tuberculous lesions.

Under normal conditions the bacilli are destroyed in the blood stream.

So far accurate investigation supports the picture we have drawn. A day comes, however, when general resistance to tuberculous invasion is lowered, either through chill or hunger or by some such shock as the baby under notice received.

Or perhaps tubercle bacilli in the bifurcation gland give rise to such destruction of tissue that the wall of a small artery or vein in the gland becomes eroded or infected and tubercle bacilli pass directly into the blood stream.

It is remarkable that Poirier has shown that veins from the bifurcation glands—or, as he calls them, the inter-tracheo-bronchial group—pass directly into the back of the great Inferior Vena Cava, the main vein from the lower limbs and trunk and thus enter the heart by the shortest possible route.

In either case, bacilli enter the blood stream in greater numbers to find there far less resistance to their multiplication or dissemination than under the conditions of health. Passing into the venous blood from the bronchial glands directly into the right auricle of the heart, they are hurried on in the heart's blood stream into the right ventricle and pumped through the

pulmonary artery into the lungs again. Most bacilli will lodge where capillary vessels are smallest and the blood stream slow; consequently tubercles are found in greatest numbers at the apex of each lung, where the movements of respiration expand the lung least and where, therefore, blood is pressed out by the lung with least force when this expands and the capillaries are dilated less than elsewhere by lung relaxation during respiration.

But lung capillaries are wide and many tubercle bacilli escape to return in the bright red oxygenated blood by the wide pulmonary veins to the left auricle. From the left auricle, the bacilli pass into the left ventricle and thence are swept in the full current of arterial blood through wide channels without a bend or branch by the internal carotid arteries to the brain. On the base of the brain, the carotid artery bends suddenly to end abruptly in branches, one of which, the largest—the middle cerebral—continues the direct line of the carotid blood stream and passing up the sylvian fissure supplies the surface of the brain.

It is precisely along this vessel that most tubercles are distributed in the variety of Meningitis which is under consideration. Hard by the spot where the internal carotid springs from the aorta the vertebral artery arises which supplies the upper cervical portion of the spinal cord. Correspondingly, the maximum incidence of tuberculous cerebro-spinal meningitis falls on the anterior aspect of the cervical spinal cord.

Next in order of infection comes the spleen, itself the blood filter, subserving a function in respect of the blood exactly similar to that of the lymphatic glands on the lymph paths. Elsewhere tubercles are found in all organs in which the blood channels terminate in minute end-arteries—vessels having no free communication with their fellows. Consequently miliary tubercles are found under the capsule of the liver and kidneys.

In miliary tuberculosis I have never seen miliary tubercles in the supra-renals nor in the pancreas, presumably because these organs possess wide blood channels in free communication with each other, as is also the case in the limbs and body wall.

Of particular interest in the case considered are the small ulcers in the intestine unassociated with tuberculous deposit in the intestinal glands.

Presumably, the baby had swallowed tubercle bacilli in his saliva as well as breathed them into his lungs; at the

end of his illness, when his resistance failed, these bacilli effected a settlement in the intestinal wall and caused ulceration.

Sometimes but far less commonly it happens that the primary focus from which tubercle bacilli find their way into the blood is situate in a mesenteric gland, the resting-place of tubercle bacilli which have reached it from the intestines. In these cases, the distribution of the bacilli in the miliary tuberculosis which ensues still remains identical with that in the case described—which originated in glands connected with the respiratory system—and is amenable to a similar explanation.

Pulmonary tuberculosis or *Phthisis Pulmonum* is by no means unknown even in very young children. Starting sometimes from a bronchial lymph gland, infected as I have described, the bacilli attack the wall of an adjacent large air tube and tubercle bacilli are sucked by the movements of breathing to all parts of the lung on the affected side, there giving rise to a tuberculous broncho-pneumonia.

It may happen too that tuberculosis will attack the lungs of a child recovering from an attack of measles or of whooping-cough.

During adolescence—the years from twelve to eighteen—attacks of tuberculosis are less frequent or severe. It would seem that the weakly ones who are either born naturally susceptible or are unduly subjected to infection from tubercle bacilli have succumbed and that the survivors are relatively a hardier race.

At this period of life the young human being becomes to a large extent independent of its parents' efforts. Learning, as experience grows, to safeguard himself from unnecessary fatigue, delighting in a life spent in the open air, his natural liking for good food in abundance is his surest defence against an organism which flourishes best on bodies vitiated by starvation. At no time is he so keenly appreciative of all in his surroundings that is conducive to enjoyment nor will he, at any other time, experience such freedom from the cares of life and the overwork which beset later years. Such factors as these are valid means of defence against an organism certainly as old as civilised humanity, which has been evolved on lines parallel with the evolution of man himself, at one time successful in the struggle, finding suitable soil in races before inexperienced, at another spreading but slowly amongst peoples long accustomed to its attacks.

When early maturity is reached, the struggle for existence becomes keener: overworked and underfed, shielded in many ways from the invigorating influence of sunlight and fresh air, both sexes then frequently succumb to the acute forms of pulmonary tuberculosis.

Two factors inseparably correlated and mutually interacting, namely a virulent infective strain coupled with a greatly diminished resistance on the part of the host, are responsible for the grave form of the disease met with under these conditions.

By far the greater number of such cases of tuberculosis are pulmonary and the distribution of the lesion usually at the apices of the lungs may be accounted for by an hypothesis similar to that above invoked to explain the distribution of miliary tubercles in the lungs.

It is noteworthy however that in the common inhalation form of pulmonary disease the lesion is not always accurately at the apex of the lung but situate slightly below and to the outer side.

The areas of lung affected earliest in the various lobes correspond also with accuracy to the distribution of the main bronchi, as has recently been pointed out by Dr. Lees.

DIAGNOSIS OF TUBERCULOSIS

Pulmonary tuberculosis in very many cases is amenable to treatment and for this reason early diagnosis is of the first importance. No surer method exists than the finding of tubercle bacilli in the expectoration of a phthisical patient but it is always to be feared that the disease is firmly established when bacilli are present in sufficient numbers in the sputum to be detected.

Many tests have been devised to ascertain the presence of a tuberculous lesion: all are open to objection, particularly in view of the extreme variation in response which individual patients show and the consequent difficulty in forming an accurate estimate of the extent of the lesion, the activity of the infecting agent and the degree of response of the patient.

Most of the tests are not without risk, in that they are either productive of immediate harmful effects or so lower the general resistance of the patient that they may reactivate foci of the disease previously quiescent. I have personal experience with

Von Pirquet's reaction alone. The test is carried out by scarifying the skin and inoculating into the abrasion a small quantity of Koch's "Old Tuberculin." The agent is a filtered glycerin-broth culture of tubercle bacilli from which the bacilli have been removed by filtration. If the reaction be positive, the skin in the neighbourhood becomes raised and red, forming a "papule."

Certain cases of tuberculous infection do not respond to the test. In miliary tuberculosis and in general tuberculosis, when presumably the various defensive mechanisms of the sufferer are completely overwhelmed by the disease, there is no reaction.

Many cases of abdominal tuberculosis and a large number of children suffering from tuberculous disease of the spine also fail to respond. The tubercle bacilli, in these conditions, give rise to very prolonged illness in which there is little disturbance in the general health of the patient. In consequence, there seems to be no general reaction on the part of the children, Von Pirquet's test failing in the absence of the substances present in the blood of patients reacting to a tuberculous infection which are responsible for the appearance of a papule after inoculation.

Tuberculous pleurisy may be cited as an example of a condition in which there is vigorous positive reaction. In this disease the onset is sudden, the illness of relatively short duration ending in recovery.

From these and other considerations based on observations on the very large number of children suffering from the varied forms of tuberculosis with which I have been associated, I am inclined to the belief that in childhood, up to the age of ten years, a positive Von Pirquet's tuberculin reaction indicates not so much the presence or absence of tuberculosis but is evidence not only that the patient has recently sustained an infection from an active strain of tubercle bacilli but that he is reacting vigorously against it.

In fact, it would seem that a positive reaction in a child suffering from tuberculosis is, for this reason, of favourable import.

After ten years of age the reaction is of little aid in diagnosis.

In old age, tuberculosis is not infrequently secondary to some pre-existing, often chronic, illness and determines its fatal ending.

NATURE OF TUBERCULOUS INFECTION

Having very briefly summarised a few of the more important results of tuberculous infection in man and indicated their special incidence in organs and groups of organs at the several periods of life, it remains to consider each of the several sources of infection in light of the evidence adduced.

Clearly, tuberculous infection—other than infection of the skin—enters the body by the air passages or the alimentary system; once in the lungs or intestine, the bacilli pass readily through lymphatic channels and reach lymphatic glands, a method of spread in many ways characteristic of children.

On the other hand, in adults, infection of the air passages is common; infection by way of the gut seldom occurs. Possibly, the strongly acid character of the gastric juice is normally a factor in promoting destruction of the bacilli.

When tuberculous ulceration of the intestine occurs in adults, it is most usually due to infection by the bacilli swarming in the sputum coughed up and swallowed by patients in the last stages of pulmonary tuberculosis.

The two possible sources of infection, however, from ingested food—particularly cows' milk—and inspired air are of importance in children. Food taken into the mouth, after mastication, is passed through the fauces into the œsophagus and swallowed, passing across the tonsils on its way to the pharynx.

It is particularly worthy of note that advanced caseating tuberculosis of the human tonsil is very rare, though caseation of glands in the neck is by no means uncommon.

Now it has been pointed out by the Commissioners on Tuberculosis that bacilli isolated from glands in the neck show *bovine*¹ characters and in their opinion there is therefore presumptive evidence that the glands were infected from milk.

Furthermore, in 15 out of a series of 96 cases in children in

¹ It should be clearly understood that by *bacilli of bovine type* is meant bacilli difficult to cultivate on media in the laboratory, capable of producing fatal disease rapidly when inoculated into animals; it is in no way implied that the bacilli are necessarily derived from oxen. The use of the term in this latter sense in the Reports has not only misled the Commissioners, but has given fictitious support to the view that milk is an effective source of tuberculosis in children. No more unfortunate expression could have been used in discussing an issue which turns entirely on the question whether or no organisms derived from cattle have been introduced into the human system.

which the tonsils showed no signs of tuberculosis to the naked eye, there was some microscopic evidence of tuberculosis.

Apparently it would seem that a strong case for tuberculous infection through the agency of milk is hereby established. It is not without significance, however, that though tuberculosis is common during the first years of life, particularly in the second and third years, advanced infection of glands in the neck is far more rare than at later periods and is seldom present unassociated with a much older lesion in the glands of the respiratory tract.

Babies fed at breast or from the bottle must suck to obtain their food and of necessity naturally breathe through their noses. On the other hand, older children, from two years upwards, soon become mouth-breathers.

Whereas air drawn in through the nose never comes into contact with the tonsils, the reverse is the case in mouth-breathers, who for this reason are constantly subject to tonsillar infections.

We are confronted therefore with the fact that, at the age when nose-breathing and milk-feeding are associated, infection of the glands in the neck is undoubtedly rare, whilst later on, when mouth-breathing is more frequent and milk no longer the only diet, the glands in the neck are frequently invaded by tubercle.

The evidence of tuberculosis in swine may be referred to here as appropriate in this connexion. It is universally admitted that swine are infected owing to their promiscuous habits of feeding. They suffer from tuberculous ulceration of the intestine and its associated lymphatic glands. They occasionally show tuberculous tonsillitis and disease of the sub-maxillary glands together with those which lie behind the pharynx and in the upper part of the neck. A form of the disease peculiar to swine is a tuberculosis spreading from pharynx to mid-ear upwards into the brain. Clearly, therefore, in cases in which the infection is definitely due to food, the tonsil is frequently implicated.

On the other hand, dogs and horses, the associates of adult man, who presumably derive tubercle bacilli from inhaled dust and the spray distributed by tuberculous stable-hands in coughing, show a large preponderance of infection of the respiratory tract—in the case of dogs 75 per cent.

Bovines suffer from both respiratory and intestinal tuberculosis. A form of tuberculous peritonitis and pleurisy to which they are subject is known as grape disease and is comparable with the forms of more chronic peritonitis in man. Large masses of tuberculous material are found studded over the peritoneum.

In about half the cases of bovine tuberculosis both forms of tubercle are present: in one-third the lungs alone are affected.

In cattle the ovaries are affected rather more often than the testicles, whereas in adult man disease of the testicles is occasionally met with, whilst that of the ovaries is very rare.

Cattle are subject to tuberculosis of the joints. Tuberculosis of the udder is a form of disease which is of particular interest from its bearing on the infection of milk. In a series of German experiments, it was found that 55 per cent. of the milk from tuberculous udders was capable of producing infection in experimental animals. Calves fed on tuberculous milk succumb to the disease and the calves of tuberculous mothers frequently become infected.

The careful experiments of the Royal Commissioners¹ on Tuberculosis prove that calves suckled by cows suffering from tuberculosis of the udder, produced experimentally by injection into that organ of tubercle bacilli from either bovine or human sources, always sustain infection of mesenteric glands and sometimes ulceration of the intestine; they occasionally exhibit tuberculosis of the submaxillary and pharyngeal glands.

Calves fed on milk containing known quantities of tubercle bacilli were proved to sustain similar lesions: the thoracic glands being also affected in some cases.

When the dose was large and the strain one found to be virulent in bovines, more extensive tuberculosis ensued, the tonsils in these cases also being affected.

Whilst it cannot be denied that milk is a possible and sometimes an actual source of tuberculous infection, especially in children, it does not necessarily follow that lesions of the intestine and its glands are a sequence of the ingestion of infected food only.

In the experiments quoted, tubercle of the thoracic glands sometimes occurred when tubercle bacilli could reach the calf

¹ Second Interim Report, 1907. Compare SCIENCE PROGRESS, No. 24, April 1912.

only in its food. It is at least possible that some bacilli, in such cases, find a resting place in the mouth and are carried thence in currents of inspired air to the thoracic viscera, eventually reaching the thoracic glands.

Similarly it may readily be supposed that air-borne tubercle bacilli, derived by children from their parents or their surroundings, may sometimes be arrested in the saliva or dissolved in the trachœal mucus and again coughed into the mouth, there to be swallowed, so reaching the intestinal tract.

The possibility that the intestine and its glands may be infected in this manner is the more readily acceptable if it be remembered that swallowed saliva does not excite the active secretion of intestinal juices containing proteoclastic ferments capable of digesting the bacilli.

On the other hand, when taken in food, tubercle bacilli are subjected to the full activity of digestive ferments. In the one case, we have a relatively concentrated suspension of bacilli in saliva permitted to act on the surface of the intestine unrestrained; in the other, bacilli are not only diluted to a great extent by admixture with food but are subjected to the destructive action of digestive juices. So far then from tuberculous milk being a necessary source, it would appear that conditions for infection of the gut in children are most favourable when food is absent.

It is not surprising, therefore, that though it is common experience to find lesions in the digestive tract associated with tuberculous infection of the lungs and thoracic glands, these lesions are often neither so advanced nor so extensive as those in the lungs.

HUMAN AND BOVINE SOURCES OF INFECTION

The exact nature of the mechanism of infection in young children must remain uncertain if judged of on the evidence afforded by considerations based on the incidence of the disease. Some other means of estimating the characters of the infection must be sought, if a clearer picture of the course of events is to be obtained.

Such a means apparently is at hand in certain differences which have long been known to exist between the majority of strains of tubercle bacilli derived from human sources on the

one hand and the majority obtained from bovine sources on the other.

The human strains grow readily on artificial media outside the body and when injected into animals produce lesions which are neither severe nor acute ; bovine strains are often extremely difficult to cultivate on artificial media but when injected into animals produce widespread and rapidly fatal disease.

The elaborate investigations of the Royal Commission have proved, as might have been anticipated, that tubercle bacilli from either source show remarkable constancy in form and behaviour during a number of generations, whether propagated by passage from animal to animal of the same or different species or by repeated subcultivations on artificial media.

There is, however, abundant intrinsic evidence in the Report that the tubercle bacillus, stable as it is, shows almost every mutation between the bovine type on the one hand and the typical human variety on the other.

Bacilli from bovine sources produce in one calf lesions of a mild or chronic type, in a second acute tuberculosis ; when the infecting agent is passed from the chronic case into another calf, acute lesions are produced, showing that there has been a gradual increase in virulence of the bacilli.

Similarly, bacilli which at first are difficult to cultivate on media will flourish after repeated subcultivation where at first they pined. In either case, the aptitude to flourish under one or other set of conditions pre-existed in some, so that the organism only required appropriate treatment to assume corresponding characters.

It is specially noteworthy that bacilli of typical bovine character have been isolated from human sources, whilst bacilli have been obtained from oxen showing a variable degree of virulence for animals and ready growth on media.

The bacilli from bovine sources show considerable constancy of character. Bacilli from human sources often vary and are placed in the Second Report of the Royal Commission in three groups, one approximating in its character to the typical bovine form, the second to the typical human form ; the strains of "group three" manifest various mutations and combinations of the characters accepted as typical in the two strains together with a marked variability in cultures and when inoculated into animals.

Bacilli from equine sources, whilst agreeing in their cultural characters with the bovine type, give rise when inoculated into animals to lesions of moderate severity such as are caused by the so-called human bacillus.

There appears to be the strongest evidence, therefore, that the bacilli of human and bovine tuberculosis are varieties of one and the same organism.

Study of variation has been far more easy in the case of the tubercle bacillus than in the case of other common pathogenic micro-organisms. The slowness of its growth, the mild chronic type of lesions which it causes, the delay which is apparent in its response to altered surroundings are all reasons which have led to an undue share of attention being given to characters probably in themselves not essential which in more rapidly growing bacteria escape notoriety.

So far as the discovery in a human lesion of bacilli either of "bovine" or of "human" type can afford evidence of infection from one or the other source, the position remains unchanged from that which existed before the work of the Commission was undertaken.

In view of the proven stability of the tubercle bacillus, it is probable that a child suffering from tuberculosis due to bacilli of "bovine" type may infect a number of other children and give rise in them to tuberculosis due also to "bovine" bacilli. In such secondary cases, it would be of little use to attack the milk supply and neglect the obvious source of infection provided by the first sufferer.

Certain of the anomalous cases, in which the bacilli exhibited both bovine and human characteristics, were attributed by the Commission to a mixed infection with the two types of organism.

In this connexion, the evidence of Von Pirquet's skin reaction mentioned before is of interest.

In carrying out this test, it is customary to inoculate tuberculins *from a bovine and a human source* on separate sites. As a result, in the vast majority of subjects, if a reaction occur, it is positive to both human and bovine tuberculin. It is incredible that in all cases a mixed infection should be operative.

In those cases in which one reaction or the other is alone positive, there appears to be no clear correlation between the site of the lesion and the type of reaction. Abdominal lesions

or lesions apparently confined to the lungs when not either positive or negative to both human and bovine tests are as often positive to the human as to the bovine reaction.

ORIGIN OF TUBERCULOUS INFECTION

In view of what has been said above it is difficult to avoid the logical conclusion that the mechanism of infection in children as in adults is in the main by way of the respiratory tract.

Children may become infected primarily by way of the intestine but there are many possibilities in addition to those afforded by infected food. Thus dirty fingers contaminated with floor dust and particles of dried sputum containing tubercle bacilli are immediately carried to the mouth of a child; or a tuberculous mother moistens with her lips the child's rubber "comforter" and bacilli are thereby conveyed to its intestinal canal.

Milk may be and no doubt is an occasional source of tuberculous infection but the importance of giving attention primarily to cows' milk rather than to other hygienic measures for the prevention of consumption is undoubtedly over-rated.

No effort should be relaxed which will serve to promote the provision of a pure milk supply from well-ordered dairies and clean, well-ventilated, well-lighted cow-sheds; at the same time, the enormously greater probability of infection from human sources, particularly from cases of pulmonary tuberculosis, cannot be exaggerated.

After all, to struggle against tuberculosis by means of measures such as are now employed or foreshadowed by legislation is at best a hopeless task. The evolution of man and of the tubercle bacillus on mutually antagonistic lines seems likely to proceed till the end of time; only by the gradual establishment of natural immunity, built up step by step by successive generations who have successfully sustained its attacks, can freedom from the disease be at last attained.

Paradox though it may seem to be, by lessening the general risk of infection of the community, the proposed isolation of persons suffering from tuberculosis may actually retard rather than assist the struggle against consumption.

What can and should be done is to place all individuals from birth onwards under conditions most conducive to the

maintenance of good health, so that they may encounter infection successfully, remembering always that overwork and underfeeding are the surest preparation for the disease.

It is not impossible that the compulsory notification of tuberculosis may have an effect perhaps undreamt of by its promoters. The unfortunate patient branded as consumptive, on the insufficient evidence afforded by the present means of making a certain diagnosis of pulmonary tuberculosis, may find himself in the position of a leper—more surely isolated by the natural fears those who encounter him may have of incurring the disease than by all the sanatoria that can be devised.

So far as children are concerned, the boiling of milk may safely be regarded as of secondary importance so long as windows are kept open and floors frequently scrubbed.

SCIENTIFIC PROBLEMS IN RADIOTELEGRAPHY

By J. A. FLEMING, M.A., D.Sc., F.R.S.

THE scientific questions that must be considered when any branch of experimental work is carried beyond a laboratory stage into the larger field of technical application are often very interesting and instructive. Apart altogether from the difficulties of conducting them on an enlarged scale or any questions of utility or profit, entirely new problems are often brought into view when we magnify the range or extent of our operations. This is true particularly of the attempts made to apply our knowledge of electromagnetic waves to long distance wireless telegraphy.

After Hertz had shown us experimentally how to produce Maxwell's electromagnetic waves and at one stroke given life to the dry bones of certain mathematical equations familiar enough to students of Maxwell's great treatise but otherwise "caviare to the general," physicists all the world over entered with unlimited delight upon the conquest of this new field of research. Laboratory experiments on electromagnetic waves became the order of the day but were carried on during five years or more before the idea arose of utilising them for telegraphic purposes. Sir William Crookes's remarkable forecast in the *Fortnightly Review* in 1892 showed that the notion of so using them had already been clearly formed; moreover, had the late Prof. D. E. Hughes not allowed himself to be discouraged by criticism of some very original experiments he snowed to friends, radiotelegraphy might have been an established fact before that date.

There is a wide gulf, however, between prognostications or suggestive experiments and a practical invention. The real invention or discovery which made possible an advance from laboratory experiments with Hertzian waves to electric wave telegraphy in any proper sense of the word was not merely an

improvement in the means of detecting these waves but the invention of a radiator which could project them far enough.

This important step in invention was made by Marconi when he constructed a form of radiator consisting of a long, nearly vertical wire insulated at its upper end and having its lower end connected to one of a pair of spark balls, the other ball being connected to the earth. What was not appreciated before his time was that a vertically arranged Hertzian oscillator of great length, say 100 feet or more, half buried in the earth had the power of producing electromagnetic waves of great energy which could travel over the earth or sea; also that a similar aerial wire would absorb the radiation and enable it to be detected. When once this clue to success had been provided, the invention of details went on apace and by 1897 or so apparatus for telegraphy without connecting wires over a range of several miles had been fairly well perfected by Marconi, whilst Lodge had also shown how the facts of electrical resonance might be applied to preserve the privacy of communication and the isolation of stations. Leaving out of account details of development and invention, for which special treatises must be consulted, we may say that at the present time (1912) the greater part of all the wireless telegraphy in the world is conducted substantially by means of the following appliances: at every station there is a transmitter and a receiver, each of which consists of three parts. The transmitter comprises: (i) some means of producing a high-tension electric current, whether by alternating current dynamo and transformer or direct current dynamo and storage cells—or in the case of small plants an induction coil and battery; (ii) a condenser, which may be a collection of Leyden jars or even a large air condenser, which is charged to a high potential by the first-named appliances and then discharged across a spark gap several hundred times a second. (iii) This sudden discharge of the condenser is sent through a coil of wire called an oscillation transformer and is made to create other oscillations of electricity in the third element called the "antenna," which is a long, nearly vertical wire, insulated at the upper end and having its lower end in connexion with the earth or with other wires laid either in or above the earth, called the balancing capacity. The antenna consists of a number of wires arranged in fan-shape or else rising up and then bent down like the ribs of an

umbrella; it may otherwise be made of a collection of wires which rise up vertically for a certain distance and then extend horizontally for a still greater length. Whatever its form, when the condenser discharges take place, rapidly reversed electric currents are set up in this antenna, each explosion of the condenser discharge producing a train or collection of such electrical vibrations in it. The antenna may be regarded as a kind of electrical organ-pipe in which electrical oscillations are produced in place of aerial oscillations; these vibrations create electromagnetic waves in the æther and these waves are projected in every direction with the velocity of light.

In the condenser or dynamo circuit there is a key or interrupter by which the trains of waves can be cut up into long or short groups in accordance with the signals of the Morse alphabet. The wireless transmitter is therefore a sort of lighthouse sending out long and short flashes of electromagnetic radiation which cannot affect the human eye but which do affect the proper kind of sensitive receiver.

Turning then to the receiver we find it also consists of three parts, viz. (i) a receiving antenna which may or may not be the same as the sending antenna; this captures or absorbs the incident electromagnetic waves, very feeble oscillatory electric currents being set up in it which are a copy on a very reduced scale of those in the sending antenna. (ii) These antenna oscillations are caused, in turn, to induce others in a nearly closed circuit comprising a receiving condenser which is tuned to the antenna; that is to say, it is arranged to have the same natural period of electrical vibration. The energy picked up by the receiving antenna is accumulated in this last circuit; hence electrical oscillations take place in this storing circuit which are an exact imitation of those taking place in the distant sending antenna and these are cut up into long and short groups which are interpreted in accordance with the Morse code. The third element in the receiver comprises the means for making these signals visible or audible. At the present time, the detectors in common use are the magnetic detector, the glow-lamp or ionised gas detector and various forms of so-called rectifying detector, the detector being associated with a telephone. If a telephone receiver alone, of the ordinary magnetic form, be connected across the terminals of the receiving condenser, no sound is produced in it unless the received electric

wave trains are extremely violent. The reason is that each train of oscillations set up in the receiving antenna by the oscillatory discharge of the condenser in the distant transmitting station consists of a group of electrical oscillations gradually decreasing in amplitude; the successive oscillations are repeated at intervals say of a millionth part of a second, some forty or fifty oscillations forming the group or train. Electrical vibrations of this frequency cannot affect the telephone, not merely because their frequency lies beyond the limits of audition but because the inductance or electrical inertia of the telephone coil is too great to permit sufficient current to flow through it at this frequency to move the diaphragm. If, however, we connect in series with the telephone some device which either rectifies these oscillations or permits movement of electricity only in one direction through it, the group of decrescent vibrations is changed into a prolonged gush of electricity entirely in one direction. If these gushes succeed each other at the rate of several hundred a second, in passing through the telephone they give rise to a musical note of the same frequency as that of the spark discharges in the transmitter.

If these latter sequences of discharges are cut up into long and short groups by a key, a listener at the telephone would hear a series of musical sounds of long or short duration, which he could interpret alphabetically on the Morse code. Many such rectifiers are now known. For instance, it is a property of carborundum—an artificial crystalline carbide of silicon made in the electric furnace—that an electric current flows more easily in one direction through the crystal than in another; consequently, as the conductivity in different directions is not the same, the crystal can act as a valve for electricity.

Accordingly, a crystal of carborundum joined in series with a telephone provides a means of hearing electrical oscillations if broken up into groups, the group frequency being preferably about 500 per second.

G. W. Pierce has found that crystals of *Hessite* (a telluride of silver) and *Anatase* (a native oxide of titanium) will act in the same manner as carborundum. Again, it has been found that a light contact between certain metals and non-metals is a better electrical conductor in one direction than in the opposite. G. W. Pickard has found that a contact between steel and silicon has this property and L. W. Austin has shown

that a contact between aluminium and tellurium will act in the same manner.

A contact between copper and molybdenite, as G. W. Pierce has shown, is likewise a rectifier, whilst Pickard has found that a contact between zincite (a native oxide of zinc) and chalcopyrite (copper pyrites) is an extremely good rectifier. This rectification does not depend on thermoelectric action, as the rectified current is generally in the direction opposite to the current which would be produced by heating the junction. R. H. Goddard has recently asserted that an oxide or sulphide film of some kind is necessary for rectification and that the contact between pure metals and pure non-metals in vacuo or hydrogen is non-effective as a rectifier.

It appears as if the film of oxide or sulphide or some other impurities on the surface permits the passage of electrons or ions through it more easily in one direction than the other when the boundary surfaces are certain metals and non-metals. In a large number of instances the direction of most easy passage is such that electrons or negative ions seem to pass more easily from the poor conductor (silicon, carbon, galena, molybdenite, pyrites, etc.) to the good conductor (steel, copper or gold) in contact with it but the tellurium-aluminium contact is an exception to this rule. In any case, there is an asymmetry of conduction at the boundary surface of many such contacts between different classes of conductors which, when associated in series with a telephone receiver, enables it to rectify trains of electrical oscillations and to make audible groups of such trains when coming at intervals of a few hundred a second.

The discovery of these rectifying contacts in the course of the search made for radio-telegraphic receivers has not only opened up questions of great interest in connexion with the conductance of electricity but has made it clear how great is our ignorance as yet concerning so familiar an operation as the movement of electricity through conductors.

In addition to the crystalline or contact rectifiers referred to, another type much used is the glow-lamp rectifier or oscillation valve, invented by the writer, which consists of a small carbon filament glow-lamp having a metallic plate in its bulb carried on a platinum wire sealed through the glass. When the filament is incandescent, the space between it and the plate has unilateral conductivity, negative electricity passing from

the filament to the plate but not in the opposite direction. This property appears to be dependent on the fact that electrons or negative ions are liberated from incandescent carbon when it is at a high temperature.

A third much used detector is the magnetic detector of Marconi, in which an endless band of hard iron wire is made to pass near the poles of a couple of small horseshoe magnets with similar poles in contiguity, the iron being embraced at that spot by two coils; one of these coils is in connexion with a telephone receiver, the other coil is free to receive electric oscillations from an antenna. The oscillations shake up the iron and either cause it to lose its quality of magnetic hysteresis or else promote an increased permeability or power of acquiring it. In either case they alter the magnetic condition of the iron within the secondary coil and hence cause an electromotive force in the latter, which in turn causes a sound in the telephone. This detector is simple and easily managed and is largely used in ship installations. The need for a form of detector which will record the signals has called forth much ingenuity. The old forms of coherer and relay and Morse printer, recording in dots and dashes on paper tape, are now very little used, owing to the numerous adjustments required and to their sensibility to external disturbances. At present, in large stations, the Marconi Company use a form of Einthoren string galvanometer joined in series with a crystal or glow-lamp rectifier. The deflections of the fine silvered quartz fibre in the strong magnetic field are recorded by photography on a prepared tape, which is developed, fixed and washed as it passes through the instrument and can record signals at the rate of fifty words or more a minute.

Provided with these detectors and the associated antenna, the radiotelegraphist is able to detect any and all sorts of electric waves passing through space.

The atmosphere round the earth is the seat of natural electrical disturbances which give rise to vagrant electric waves, called atmospheric X's or strays, which are recorded on the receivers used in competition with the message-bearing waves sent out from transmitting stations in correspondence with the receiver. In early days, when the coherer was used in conjunction with the Morse printer as receiver, these atmospherics were difficult to eliminate; they gave rise to false signals, dots

and dashes, which mingled with the intelligible signals and confused their meaning. Now-a-days, by the use of a high-spark frequency, the intelligible signals have a high shrill note in the telephone and it is therefore possible to distinguish them from the lower clicks, knocks or squeaks produced in the telephone by the atmospherics. And, in addition, better forms of tuning circuits have been devised for getting rid of the more highly damped atmospherics. A study of these atmospherics will undoubtedly lead to the increase of our knowledge of atmospheric electricity; indeed, a considerable amount of information on this subject has already been accumulated. Natural electric waves are produced whenever a lightning discharge takes place, as this is just of a nature to produce a sudden disturbance in the æther like the wave caused in the air by an explosion. But as these stray waves are always found flying across country, whether local thunderstorms are going or not, there must be some constant source from which they come; it may well be that they arise in the tropical regions of the earth and are propagated to temperate zones. On the other hand, although stray electric waves can nearly always be heard in the telephone of a radiotelegraphic receiver, in our latitude they are much more frequent by night than by day; moreover, Mr. Marconi and Dr. Eccles have noticed that they undergo a very curious stoppage or suspension just about sunset and sunrise. The latter investigator has described the twilight course of these stray electric waves in England in the following terms:

"Starting to listen (at the telephone) about a quarter of an hour before sunset (in London) on a favourable afternoon in late autumn or winter, the strays heard in the telephone are few and feeble, as they have been all day; then at five minutes after sunset a change sets in, the strays slowly get fewer and fewer until at ten minutes after sunset a sudden distinct lull occurs and lasts perhaps a minute. Often, at this period, there is a complete and impressive silence. Then the strays begin to come again and quickly gain in number and force and in the course of a few minutes they settle down into the steady stream of strong strays proper to the night."

In tropical countries great irregularities are noticeable but broadly we may say that, at any place, these stray electric waves are subject to regular diurnal variations something like those of atmospheric electric potential or terrestrial magnetic force;

moreover, irregular disturbances caused by local storms are superposed upon these diurnal variations. Many of the stray waves observed in England affect radiotelegraphic stations hundreds of miles apart nearly simultaneously and therefore cannot have their origin in England.

We have next to notice the diurnal variation in the strength of artificial or message-bearing waves sent over long distances. The fact was discovered by Marconi in 1902, in one of his voyages across the Atlantic, that whilst by day he could (using a certain receiver) only receive signals from his Cornwall station at a distance of about 700 miles, he could receive similar signals at a distance of 2,100 miles by night. The first hypothesis suggested was that this was due to the discharging power of daylight upon the sending antenna. The daylight effect, however, is only a long-distance effect and no such reduction in the strength of day signals is noticed over short distances. Hence it cannot be an effect produced merely on the sending antenna. It was then suggested that the atmosphere became ionised by the sunlight and that this was the cause of the absorption of the electric waves. We can calculate the absorption due to any amount of assumed conductivity in the air when long electric waves are passing through it and it is not difficult to prove that the atmospheric conductivity which has been observed at sea level or a few hundred or even thousand feet above it is not sufficient to account for the observed diminution of range of long electric waves by day as compared with night, if it be attributed to mere absorption of wave energy, in other words, to want of transparency due to some degree of conductivity in the air.

There is, however, another possible explanation. It is well known that sound travels better with the wind than against it. The late Sir George Stokes explained this as follows: When the wind is blowing strongly, friction against the earth retards it more at the surface than at higher levels. If then a plane vertical sound wave-front be travelling against the wind, the velocity of the wave will be more reduced at higher levels than at the earth's surface. Hence the wave-front is no longer vertical but slopes backward. The direction of propagation of the wave being normal to the wave-front, the wave is tilted upwards and the sound will pass above a distant point on the terrestrial surface which it would otherwise reach if the wind

were not blowing against it. A similar effect is possible in connexion with electromagnetic waves passing through air. It has been shown by Dr. Eccles that, on certain assumptions as to the nature of the ions, an electromagnetic wave travels faster in ionised air than in non-ionised air. It has also been proved experimentally by the writer that air containing condensed moisture, in the form of water spherules, has a slightly higher dielectric constant than dry air. Hence the wave velocity is slightly less in moist than in dry air. Also, there is experimental evidence for the statement that ultra-violet light can ionise air and separate from the molecules positive and negative ions. Owing to the rapid absorption of the ultra-violet light of the sun by the atmosphere, this kind of ionisation is principally confined to layers of air at a considerable height, in fact above the level of ordinary clouds. We have then the necessary conditions for producing a refractive effect. The electromagnetic waves radiated from an antenna are sent off with greatest intensity in a horizontal direction but radiation takes place to some extent in directions elevated above the horizontal. When these upward-trending waves reach the ionised layer of the atmosphere, owing to the greater velocity of the upper part of the wave-front, a refractive effect takes place which bends them down again. The effect may be compared with an inverted mirage effect, the layers of ionised air corresponding to the layers of hot air and the layers of non-ionised air to the cold air. Again if the atmosphere from any cause become ionised in patches, such non-homogeneous air would behave to electromagnetic waves as water full of bubbles behaves to light; it would become more or less opaque and break up the wave-front passing through it.

Before applying this theory in explanation of observed facts, it will be well to turn attention for a moment to the fundamental scientific question in connexion with long-distance radio-telegraphy, viz. why is it possible to send electromagnetic waves round the earth over long distances? Suppose we consider an analogy with light. If a luminous point of mathematical dimensions were placed on the pole of a sphere one-quarter of an inch in diameter, the radiant light would be diffracted to a very small extent round the sphere but certainly not to such a degree as to illuminate the sphere at its equator or even at 45° latitude.

Yet the length of these visible light waves bears the same relation to the diameter of such a small sphere that radiotelegraphic waves one kilometre in length bear to the diameter of the earth. Hence it is by no means obvious that Transatlantic radiotelegraphy is conducted in virtue of the diffraction of these long waves. The matter has, however, been more carefully examined by mathematicians—by Lord Rayleigh, the late Prof. Henri Poincaré, Prof. H. M. Macdonald and Dr. Nicholson. They have all come to the conclusion that radiotelegraphic waves of even a kilometre or more in length cannot be diffracted round the earth to an extent sufficient to account for Mr. Marconi's long-distance wireless telegraphy. Hence our fundamental problem is to find a valid reason for the propagation of these long electric waves in spite of the curvature of the earth across the Atlantic or even, as achieved by Mr. Marconi, from Ireland to South America, a distance of 6,000 miles or one-quarter of the way round the earth. A mathematical discussion of the problem has made it tolerably clear that if the earth were a ball of copper immersed only in æther, no long-distance radiotelegraphy would be possible on it. The waves generated at any place would soon glide off it and be lost in space. The fact that we can conduct such telegraphy on it over long distances is only due either to the imperfect conductivity of the earth or to its possessing an atmosphere of such a nature that electric waves created on it are prevented by some means from rushing off it tangentially into space. One explanation has been formulated by Prof. A. Sommerfeld of Munich as the result of an elaborate mathematical discussion of the problem of wave generation by an oscillator at the boundary of two dielectrics of different kinds. His conclusion is that, in the case of an oscillator at the bounding surface of earth and air, there is not only an electromagnetic space wave radiated through the air but a surface wave which travels along the bounding surface and is limited to a small region on either side. There is a certain analogy with a similar effect in the case of earthquakes, in which, as investigation has shown, there are space waves travelling through the earth and surface waves more or less confined to the surface crust. Sommerfeld shows that these surface waves would degrade in amplitude much less fast than the space waves and would follow round the surface of the earth in spite

of curvature and irregularities. His suggestion is that long-distance wireless telegraphy is chiefly effected by such surface waves. Although his analysis is no doubt valid, yet nevertheless the trend of experimental evidence seems to be against it.

If Sommerfeld's explanation were the true one, it is hard to see why long-distance wireless telegraphy should either be so much affected by daylight and by direction or exhibit the abnormalities with regard to wave length which it actually does.

There remains then one other assumption for which the evidence is far from complete but which has been the ultimate refuge of all those who have found it impossible to account for the facts either on the basis of diffraction or on the hypothesis of a surface wave. This assumption is that the upper levels of the earth's atmosphere, say at a height of 60 to 100 miles, are perpetually in a state of ionisation to such a degree as to render it a fairly good conductor, possibly as good as dilute sulphuric acid, at any rate sufficiently good to enable it to act as a reflector for long electric waves.

Since pure wave diffraction is excluded as a possible explanation of long-distance wireless telegraphy, we have to fall back on one or other of two hypotheses, one of which requires the production of surface waves in the crust of the earth and the other refraction or reflection of waves in or by the earth's atmosphere. As will be seen by what follows, the abnormalities and irregularities of long-distance radiotelegraphy seem to point to the determining cause of the bending of the waves round the earth being something in the atmosphere rather than in the earth. Yet, on the other hand, the existence of surface waves is not disproved and there are some facts which rather strongly support this latter supposition. The experimental achievements and chief practical experience with long electromagnetic waves which have to be explained are therefore as follows: electric waves from 1 to 4 miles in wave length can travel at least one quarter of the way round the earth. This suggests at once the questions—Could they travel half-way round? Is wireless telegraphy between London and New Zealand within the possibility of practice? In the next place, there are great differences in the reduction of amplitude experienced by such a wave when travelling by day and by night over long distances; and certain extraordinary variations in the strength of signals near sunrise and sunset.

Again, as observed by Mr. Marconi and his staff, there are great differences in the facility with which these waves are propagated in different directions, as in some cases they are more easily sent in north and south directions than in east and west. Lastly, we have the same influence exerted by daylight on the stray or natural waves as on the message-bearing waves.

Under some conditions of the atmosphere, radiotelegraphic apparatus intended for moderate distances will send or receive over unusually great distances and these "freak transmissions," as they are called, are more likely due to some abnormal state of the atmosphere than of the earth. Hence these vagaries of transmission can hardly be explained merely by a surface wave or by any regular process of transmission of wave energy.

It is clear that the unravelling of the knotty problems of radiotelegraphy is bound up with a much more complete insight into the structure of the earth's atmosphere. It is indeed curious how, as progress takes place in science, blows are continually struck at our complacent ignorance. Time was when we all confidently thought the earth's atmosphere was merely a mixture of oxygen and nitrogen with a dash of carbon dioxide and some aqueous vapour; then suddenly we learnt that it contains argon, neon, helium, zenon, krypton and perhaps traces of several other gases. Radiotelegraphy is now teaching us that it is of a still more complicated character and possesses constituents which have the property of refracting, perhaps reflecting and also absorbing, long electromagnetic waves in an extraordinary manner.

Many of the mathematicians who have attacked this problem of the bending of long electric waves round the earth have fallen back on the hypothesis that there must exist at a high level of the atmosphere a layer of rarefied gases which are so much ionised that they form a very good conductor of electricity, possibly as good as dilute sulphuric acid at its maximum conductivity. This layer is assumed to have the property of producing, by an inverted mirage effect, a reflection of long electric waves. The hypothesis of long-distance transmission that is then suggested is something as follows: When a radiotelegraphic station is at work, the waves sent out horizontally are diffracted to a small extent and may reach the receiving stations at a very few hundred miles' distance directly. In

addition to these horizontal radiations, rays are sent upwards at various inclinations which impinge on the reflecting layer and "illuminate" it, radiotelegraphically speaking, just as a distant conflagration so far off as to be below the horizon illuminates clouds in the sky or "lights up" the sky, to use Dr. Eccles's expression.

The suggestion is that, at very great distances, the waves received, at least at night, are these reflected waves. In the daytime, it is assumed that there is an additional ionisation, by ultra-violet sunlight, of the air at still lower levels and that the effect of this in accelerating the wave velocity of the upper part of the wave front travelling through it is to bend down the rays again earthwards, so as to make them fall short of the distant receiving station. Perhaps also the interposition of the middle layer of ionised air may reduce the perfection of the reflection by the upper permanently ionised air. The two effects combined are postulated as an explanation of the reducing action of daylight on radiotelegraphy. Since the sunlit half and the dark half of the atmosphere are in different conditions as regards ionisation, at the boundary line there will be a more or less confused state which might be likened to a liquid in a state of froth: hence the propagation of a wave across the boundary line is accompanied by difficulties or obstructions which do not affect transmission in the more homogeneous night half or day half of the atmosphere.

Turning then to the facts of observation as regards the day and night effects, some careful observations were made by Messrs. Round and Tremellen at the Marconi Company's works at Chelmsford last year, in July, which are recorded in a recent issue (November 1912) of *The Marconigraph*. They observed at Chelmsford the strength of the signals sent out from the Marconi station at Clifden in Ireland through a whole day and night. Beginning say at midday, the strength of the signals received at Chelmsford remained tolerably constant until about an hour before sunset at Chelmsford. It then rose quickly to about four times its day strength at a little after sunset at Clifden. This rise was then followed by a sudden fall off in strength again, which reached a minimum about an hour after sunset at Clifden. About an hour later a very sudden increase in strength set in which carried up the signal strength to nine or ten times its minimum day value; this continued with

some irregular variations during the night. About an hour before sunrise at Chelmsford, there was another sudden decrease, followed by a rise and then a fall to normal day strength soon after sunrise at Clifden. There is therefore one maximum about sunset at Clifden and another at about sunrise at Chelmsford.

Mr. Marconi pointed out in a Royal Institution lecture at the same date that in Transatlantic radiotelegraphy the signals are at their weakest when the boundary between day and night has moved into a position about half-way between the two stations on opposite sides of the Atlantic.

Other observers, such as G. W. Pickard, also have noticed this curious dip or minimum in the signal strength curve at or about sunrise and sunset. Similar variations are found to affect the wave strength of the natural or stray waves. There are, however, great variations in the phenomena due to wave length and the position of the two stations in correspondence. Hence we are very far yet from being able to lay down simple general statements as to the facts or fit them to equally simple explanations.

Thus in the case of the Transatlantic transmission during the day, conducted with waves 4,000 metres in length and passing from Nova Scotia to Ireland, Marconi states that the waves yield strong and steady signals during the day at Clifden (Ireland) which gradually decrease in strength after sunset, reaching a minimum about $1\frac{1}{2}$ hours afterwards; they then increase again until sunset at Cape Breton (Nova Scotia) and attain later on a high maximum value. During the night they vary a good deal in strength. Shortly before sunrise at Clifden the signals grow stronger and decrease to a lower value about two hours later; they then return to normal day strength.

Beyond a distance of 4,000 miles, signals have only been received by night but Mr. Marconi has remarked that it is curious that the signals sent out from Clifden should only have been detectable in Buenos Ayres by night, whereas in Nova Scotia they are no stronger at night than in the day. He has also noted the curious fact that whereas ships 1,000 miles from England off the south of Spain or round the coast of Italy can nearly always communicate with post office stations on the British coast by night, yet the same ships when at an equal distance away in the Atlantic or to westward cannot

communicate except with the aid of especially powerful apparatus. It will be seen, therefore, that we have by no means as yet even determined all the abnormalities of the effects, far less reached a final explanation of them.

There are many geophysical facts which seem to indicate that the upper layers of the earth's atmosphere are in a highly conductive condition. We have in the first place the phenomena of the Aurora, which is generally allowed to be an electrical discharge and although its principal manifestation is in higher latitudes, yet, as W. W. Campbell showed in 1895, the green auroral line λ 5,770 can be seen on moonless nights in any part of the sky. Then, again, we have Prof. Schuster's conclusions from observations of terrestrial magnetism that the greater part of the diurnal variation must be due to electric currents in the upper atmosphere; also the suggestions of Arrhenius and of W. J. Humphreys that the outermost layers of our atmosphere are kept permanently ionised either by electrons shot out from the sun or by the bombardment of cosmical dust. Prof. Schuster's conclusion is that at a height of about 100 km. the atmospheric conductivity is of the order of 10^{-13} electromagnetic units, equivalent to 10,000 ohms per centimetre cube; Dr. Eccles, working from this basis, finds that this conductivity would suffice to give sufficient refractive power equivalent to reflection for radiotelegraphic waves.

Hence it appears as if a complete explanation of long-distance radiotelegraphy and of the variations introduced by daylight is intimately bound up with a knowledge of the state as regards conductivity, dielectric constant and ionisation of the air at very high levels. Our methods of direct exploration by balloons are probably limited to altitudes of 7 or 8 miles, hence all our knowledge of effects and states at a height of 40 or 50 miles will have to be derived by inferences drawn from observations of terrestrial magnetism, electricity and geophysics generally.

Furthermore, recent experiments by Dr. H. Löwy and others have shown that electromagnetic waves may be used to explore the crust of the earth and perhaps locate masses or veins of metallic nature. A better knowledge of the function of the earth in wireless telegraphy is therefore necessary as a basis for theory.

Sufficient will have been said in this article to show that whilst radiotelegraphy has proved to be a weapon of enormous value for supermarine intercommunication, for saving life at

sea and rendering more secure the position of those who have to brave the perils of the deep, it has opened up scientific questions of remarkable interest, which dovetail in with other unsolved problems of terrestrial physics and invite the careful consideration of expert students in many branches of physics. It is curious how frequently the achievements of inventors outrun all our powers of explaining the inventions in terms of accepted knowledge. The unconscious cerebration of genius attempts and succeeds but the exact reasons for success are sometimes hard to find. Though we do not yet quite know why it is possible to send electromagnetic waves across the Atlantic, the fact that it can be done has increased to a most valuable extent the means of communication on which the conditions of our modern life and even our national welfare incontestably depend.

X-RAYS AND CRYSTALS

By W. L. BRAGG, B.A.

INTRODUCTORY STATEMENT

SINCE Röntgen Rays were first discovered, many experiments have been made to obtain with them some effect analogous to the interference, diffraction and reflection of light waves but till quite recently it could be said of all these experiments that they gave a negative result. X-rays are scattered and absorbed by bodies placed in their path but this scattering and absorption have been found to depend merely on the nature of the atoms of which the body consists and no evidence has hitherto been forthcoming of any influence due to the chemical combination or physical arrangement of these atoms. Such effects as interference and reflection demand a wave front covering a large area, in order that the arrangement of lines in a grating or the plane surface of a mirror may impress its nature on the wave. It has seemed that an X-ray represents energy limited to so small a volume as to be concerned merely with the nature of single atoms traversed by it, so that it could not be reflected by a mirror, however perfect the polish, because it had no means of distinguishing between smooth and rough.

The experiments which form the subject of this article have quite altered the aspect of the problem. An effect has been obtained which shows that the regular arrangement of atoms in a crystal makes its impress on the rays from an X-ray bulb traversing the crystal. Not only is this so but the effect can be explained on any wave theory whatever, with suitable assumptions about the wave lengths; it is apparently due to the interference of waves of the normal type having energy spread continuously over a wave front. Except for their extremely small wavelength, they are in all respects like waves of light and heat. If these radiations are identical with the X-rays as investigated by ionisation methods, there is the same paradox with regard to their "corpuscular" and "wave" nature as there is in the case of ultraviolet light. The transformation of X- into cathode-rays and *vice versâ* can be observed several times over

with the rays from a bulb and it is almost incredible that the reappearance of a definite amount of energy associated with a cathode ray in these transformations should not be due to the energy also being associated with the intermediate X-ray. Yet this seems impossible when the energy of the X-ray is spread over a wave front. However, the more paradoxical the case seems, the more interesting it becomes; indeed, there can be no doubt that this new effect must go far towards solving that puzzling problem, the nature of X-rays.

Electromagnetic waves are now known to us of all wave lengths over a range of many octaves. When Hertz first obtained the electromagnetic vibrations predicted by Maxwell in his theory of light, there was a vast gap between the wave lengths corresponding to the frequencies of his oscillators and those of visible light, the former being something like a million times the latter. This gap has now been narrowed until it can be said to have been abolished. On the one hand, the investigation of the spectrum of light from hot bodies has been pushed far into the infra red, heat waves of longer and longer wave length being discovered by means of a radiomicrometer. These long waves are isolated by different methods, such as continued reflection on a surface of rock salt or sylvite or by making use of their strong refraction by quartz; their wave length may be found by interferometer methods. In this way, Rubens and Wood have been able to show the existence of heat waves as long as $\frac{1}{16}$ mm. in the radiation from a Welsbach burner.

On the other hand, by the use of improved forms of oscillators, very much shorter electromagnetic waves have been got than those which Hertz investigated. The shortest waves as yet obtained have a wave length of 2 mm. Thus there is hardly any gap left in the spectrum and one may now say that all wave lengths greater than those of visible light are at our command.

The wave length of visible light lies between 4×10^{-6} cm. and 7×10^{-6} cm. and the known range of the spectrum extends on the other side to the smaller waves composing ultraviolet light, the region investigated photographically. Here waves as short as 1×10^{-6} cm. have been found by Schumann and are named after the discoverer. Until quite recently, the spectrum as known to us has ended at this wave length.

However, the experiments performed by Messrs. Friedrich, Knipping and Laue have opened up a vast new range in the spectrum never explored before. The paper in which they announce their results appeared in June 1912, in the Proceedings of the Royal Bavarian Academy of Science. The effects which they obtain can only be ascribed to waves of a length of the order of one hundred millionth of a millimetre, the wave length being small compared with the accepted radius of an atom!

When dealing with visible light, a diffraction grating is most commonly used in order to split up the mixture of light of all colours into components the wave length of which can be measured. The effect produced by the diffraction grating is a consequence of the regularity of its structure; it is ruled with lines at constant intervals which are greater than the wave lengths of the light to be examined but of the same order of magnitude. It is the interaction of this regular spacing of the lines and that of a train of waves composing monochromatic light which leads to the appearance of interference maxima and minima. Now there are reasons to suppose that X-rays consist of electromagnetic waves of very short wave length, something of the order 10^{-9} cm. Laue came to the conclusion that if this were so, it might be possible to get interference effects with these short waves by using a crystal as a diffraction grating. The atoms of a crystal are regularly arranged and on the whole the intervals between them bear about the same relation to the wave length 10^{-9} cm. as does the "constant" of a diffraction grating to the wave length of visible light. To these waves a crystal is really a most perfectly ruled grating. The experiments to test this prediction were carried out by Friedrich and Knipping at Laue's request: they obtained a positive result with the first crystal they tried.

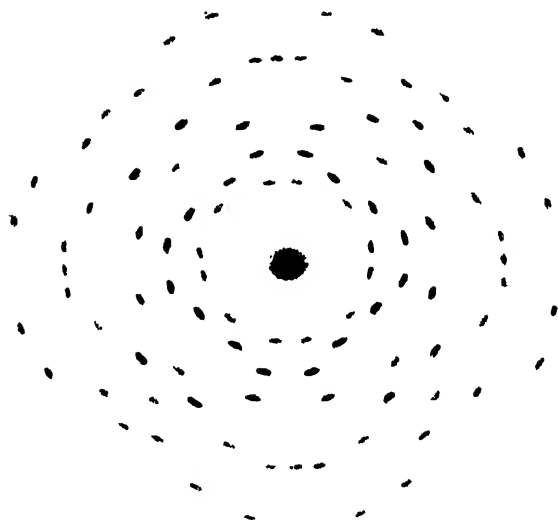
Since no way has yet been devised of obtaining a parallel beam of X-rays corresponding to the parallel light which falls on the grating in a spectroscope, an approximation to this must be made. By a series of fine holes in screens of lead, the X-rays from a bulb were stopped down till a very narrow pencil 1 mm. in diameter was obtained.

This was allowed to fall on a small crystal of copper sulphate and about 3 cm. from the crystal, in the direction away from the bulb, a photographic plate was set perpendicular to the beam of X-rays. The experimental arrangements are shown in fig. 1.



A

A. Interference pattern obtained with crystal 1 in blende; the direction of the incident ray is $\frac{1}{2}111$, parallel to trigonal axis of the crystal.



B

B. Diagram representing the pattern obtained with the same crystal when the incident rays are parallel to a cubic axis. In this diagram the fainter spots have been made more distinct.

FIG. 2

This beam was by no means completely absorbed by the crystal ; it traversed it to fall upon the plate and when the plate was developed after several hours' exposure, the effect produced was visible as a circular dark spot of much greater dimensions than the cross section of the incident beam on account of slight scattering of the rays. But besides this intense spot, there appeared around it, on the plate, a series of much weaker spots apparently arranged in a geometrical pattern. Fig. 2 shows two typical crystallographs obtained with a crystal of zinc blende. By altering the distance of the photographic plate from the crystal,

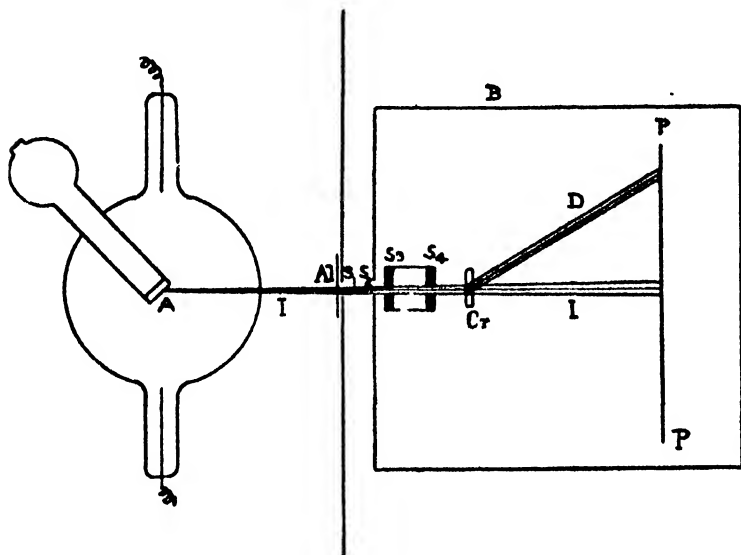


FIG. 1.

A, Anticathode. Al, Aluminium window. S₁, S₂, S₃, S₄, Stops. Cr, Crystal. P, P, Photographic plate. B, Light tight box. I, The incident beam. D, A diffracted beam.

the small spots could be made to close into or move out from the big central one and it was clear that they were formed by narrow rectilinear pencils spreading from the piece of crystal. Some of these pencils make quite large angles, as much as 40°, with the direction of the undeviated ray. The spots hardly altered in size as the distance of plate from crystal was increased, remaining always of the same size as the smallest stop. Copper sulphate forms triclinic crystals, belonging to one of the more complicated systems ; in order to obtain results which could be analysed more readily, a crystal was chosen which belongs to the

more simple cubic system. As it seemed possible that the effect was due to a secondary X-radiation, it was deemed advisable to use a crystal containing one of the heavier metals which give much secondary radiation; for this reason zinc blende was the crystal chosen.

Fig. 2B shows the result obtained when the beam of rays traverses a crystal of zinc blende in the direction of a cubic axis of symmetry, the interference pattern on the photographic plate showing complete fourfold symmetry.

The interference maxima are little elliptical spots arranged in a complicated geometrical pattern; these spots represent narrow rectilinear pencils spreading from the piece of crystal traversed by the rays. From a knowledge of the position of one of the spots on the plate and of the distance of the plate from the crystal, it is easy to find the direction of the pencil which formed the spot in question; and taking the cubic axes of the crystal as axes of reference, to define the direction in terms of the angles made with the axes. As the incident waves pass through the crystal, they act on the atoms which they meet, a secondary wavelet spreading from each atom as a wave passes over it. If the incident beam contain a train of waves of wave length λ , then in order that there should be an interference maximum in a particular direction it is necessary that the train of wavelets from every atom in the crystal should be in phase in that direction.

To express this condition analytically, some assumption must be made as to the arrangement of the centres from which the secondary wavelets spread.

Laue regards these centres as forming a point system which has for its pattern a little cube with a point at each corner. This is the most simple cubic point system possible. Take for convenience axes of reference parallel to the cubic axes and origin at the centre of one of the atoms, molecules or whatever it may be that represents the diffracting unit; then the neighbouring atoms will be equally spaced in all three directions OX, OY, OZ. Let the incident light be parallel to the axis OZ and the distance between neighbouring atoms be a .

If we express the condition that the wavelets from the atom at the origin should be in phase with those from its nearest neighbours along OX, OY, OZ, we ensure that the wavelets from all the atoms in the crystal are in phase.

This may readily be shown to be so if:

$$\begin{aligned} a\alpha &= h_1\lambda \\ a\beta &= h_2\lambda \\ a(1-\gamma) &= h_3\lambda \end{aligned} \quad (1)$$

where h_1, h_2, h_3 are integers and α, β, γ the cosines of the angles which the direction considered makes with the cubic axes.

This is analogous to the equation which holds for a line grating:

$$\lambda \sin \theta = n\lambda$$

where n is the order of the spectrum 1st, 2nd, 3rd, etc., a the "constant" or interval between successive lines and θ the angle which the direction of the telescope axis makes with the normal to the grating when a line of wave length λ lies on the cross wire. It means that the wave from the atom at the origin is h_1 and h_2 wave lengths behind that from its neighbours along OX and OY respectively, h_3 ahead of that from its neighbour along OZ .

These equations can at once be tested. By knowledge of the position of a spot on the photographic plate, the $\alpha\beta\gamma$ of its pencil can be calculated and since from equation (1)

$$\frac{\alpha}{h_1} = \frac{\beta}{h_2} = \frac{1-\gamma}{h_3}$$

the values of $\alpha, \beta, 1-\gamma$, for the spot ought to be in a simple numerical ratio. As a matter of fact, this is found to be so; the values of $\alpha, \beta, 1-\gamma$ for spots are in ratios such as 1:3:1 or 1:9:3. In no case is it necessary to assume a number h_1, h_2 or h_3 greater than 10 in order to fit these values of $\alpha, \beta, 1-\gamma$ to a whole number ratio. This affords strong confirmation to the theory that the spots are due to interference.

The numbers h_1, h_2, h_3 are the most convenient parameters with which to define an interference maximum. They give at once the position of the spot on the photographic plate and the wave length to which it corresponds.

By choosing for h_1, h_2, h_3 any three integers, one obtains the position of a spot which ought to appear in the photograph, if the incident radiation contain the wave length corresponding to these three integers.

To each spot in the photograph in fig. 2 numbers h_1, h_2, h_3 can be assigned in this way. If this be done and the numbers corresponding to the most marked spots are arranged approxi-

mately in the order of their intensities, some spots being much darker than others, the following list is the result :

TABLE I

h_1	h_2	h_3		h_1	h_2	h_3
5	3	1		2	2	1
1	5	1		1	3	1
2	3	1		5	5	1
1	4	1		1	7	1
3	3	1		7	7	3
0	3	1		3	7	3
1	9	3		1	5	2

Each set of numbers in this table corresponds of course to four or eight spots in the pattern, according as h_1 and h_2 are equal or unequal.

I.e. 5, 3, 1 represents $\pm 5 \pm 3 \pm 1$

$\pm 3 \pm 5 \pm 1$

3, 3, 1 represents $\pm 3 \pm 3 \pm 1$

the pattern being of fourfold symmetry.

Though all the numbers are simple ones, it is not at once obvious that they belong to any system. Why, for instance, should there be spots in the photograph for which h_1, h_2, h_3 have values 1, 3, 1; 1, 4, 1; 1, 5, 1; 1, 7, 1; and no spot corresponding to 1, 6, 1? Also there are sets 2, 2, 1; 3, 3, 1; 5, 5, 1; but no set 4, 4, 1. There are many similar gaps in the series. Again the most intense spots are not given by the simplest value of h_1, h_2, h_3 in this list, as would seem natural, these spots being analogous to the spectra of low orders in the case of a diffraction grating, which are generally the brightest. A theory of the effect must attempt to explain these anomalies.

On considering equations (1), which for convenience are here repeated :

$$a\alpha = h_1\lambda$$

$$a\beta = h_2\lambda$$

$$a(1 - \gamma) = h_3\lambda$$

it is clear that a knowledge of the numbers h_1, h_2, h_3 to be assigned to any spot determines the wave length of the radiation which has at that point an interference maximum, as well as the direction cosines of the pencil which forms it. There are three equations to be satisfied. The values of $\alpha\beta\gamma$ represent only two variables since they must obey the equation :

$$\alpha^2 + \beta^2 + \gamma^2 = 1$$

and therefore the value of λ must also be adjusted to satisfy the equations. It is here that the action of a "three dimensional" grating differs from that of a "one dimensional" grating like an

ordinary line grating. In the latter case every wave length can form an interference maximum, in other words, the grating can give a continuous spectrum. In the former case two extra conditions must be satisfied, only one more direction cosine is available and now it is only certain wave lengths that can form maxima at all.

A similar effect may be got with a line grating and white light. If half-silvered parallel plates are placed in front of the grating of a spectroscope, thus introducing an extra condition for interference, a continuous spectrum is no longer obtained when white light is focussed at the collimator, but in its place is seen a line spectrum representing a series of definite wave lengths. If for the line grating a cross grating were substituted and for the collimator slit a small hole at its centre, the analogy would be still closer.

If the photograph represent the most general pattern possible, all values of h_1 , h_2 , h_3 ought to correspond to spots. This is of course impossible; consequently there must be some limit to the values of h_1 , h_2 , h_3 and in a general way it may be said of the actual photograph obtained that the larger these numbers are, the fainter are the corresponding spots. But at any rate, it would be expected that the spots in the photograph should correspond to a list of numbers h_1 , h_2 , h_3 complete over a certain range. This is not so and some explanation must be put forward to explain why certain spots fail to appear.

The explanation which Laue suggests is this—that when a spot corresponding to some simple set of numbers h_1 , h_2 , h_3 is missing in the photograph, it is because there is absent from the incident radiation that wave length which is the only one capable of forming the spot in question. On the other hand, certain other spots do actually appear, because the right wave lengths to form them are available. In his paper, he shows that all the prominent spots in the photograph can be explained as due to five wave lengths which may be regarded as five broad lines in the spectrum of the incident radiation. The lines must be broad because the five definite wave lengths only satisfy the equation approximately. This explanation is not very satisfactory. In the first place it invokes the aid of five constants to explain the pattern and in the second place these five wave lengths would give many other spots which, as a matter of fact, do not appear in the photograph.

There is another way of explaining why certain spots fail to appear and I think that by its means the whole pattern may be shown to be far more general than Laue considers it to be. The point system in which the atoms are arranged for the purpose of the above analysis is not the correct one.

Point systems of cubic symmetry have three elementary forms. There is that taken by Laue which has, as element of its pattern, points at the corners of a cube; another which has points at the corners and one at the cube centre; a third with points at the corners and also at the centres of the six cube faces. It seems to me that it is this last system which is revealed as characteristic of the structure of the zinc blende crystal by the interference pattern. This different point system involves a

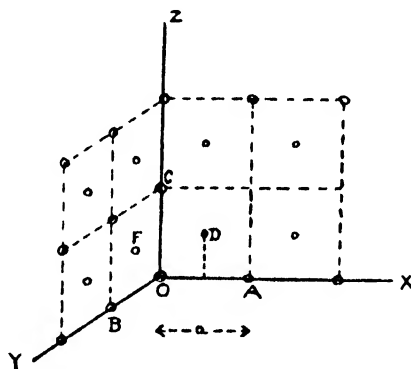


FIG. 3.

slight change in the analysis. Suppose, as before, that axes are taken with origin at an atom and that the atom at the origin send off a wavelet which is in the direction $\alpha, \beta, \gamma, h_1$ wave lengths behind its neighbour along the x axis and so forth. The distance between neighbouring atoms along the axis is "a" as before but this is no longer the shortest distance between atoms.

The arrangement of the atoms in the XZ and YZ planes will be as in fig. 3. The previous equations (I) ensure that all atoms such as O, A, B, C, etc., shall emit wavelets in phase. We must now express the condition that atoms at points such as D, F, the extra atoms at the centres of cube faces should also emit wavelets in phase with those from O.

The difference in phase of wavelets from D and O will be

$\left(\frac{h_1}{2} - \frac{h_2}{2}\right)$ wave lengths, the co-ordinates of B being $\frac{OA}{2}, \frac{OC}{2}$; this must be a whole number. In order that this may be so, the numbers h_1 and h_2 must be both odd or both even. The same condition must hold for h_2 and h_3 . This at once explains the peculiarities of the list of numbers h_1, h_2, h_3 . Taking in that list all the cases in which h_3 is unity, by the above rule h_1 and h_2 must be odd. It is now clear why in so many sets h_1 and h_2 have odd values and are even in two cases only, *i.e.* 1, 4, 1; 2, 3, 1. These last must now be written 2, 8, 2; 4, 6, 2 and are comparatively complicated.

- If the numbers h_1, h_2, h_3 in the table be reconsidered, assuming this new arrangement of the "elements" of the crystal grating, the reason of their selection becomes clear. Take the sets of numbers which have $h_3 = \text{unity}$. In the first place, by calculating the corresponding wave lengths, they can be shown to consist of every possible set of numbers which correspond to wave lengths greater than a limiting value $\lambda = \cdot 034a$. In the second place, sets corresponding to a wave length approaching $\lambda = \cdot 06a$ give the two very intense spots 1, 5, 1 and 5, 3, 1 which form a marked inner square in the photograph; when the wave length corresponding to a spot is greater or less than this value, the spot is more faint, until spots corresponding to the limiting wave length $\lambda = \cdot 034a$ can hardly be seen. It is as if the incident radiation had a continuous spectrum with a maximum intensity at the region $\lambda = \cdot 06a$. If now the sets of numbers having $h_3 = 2$ be considered, exactly similar results are obtained. There are two very intense spots which may be designated as 4, 6, 2 and 2, 8, 2, which have wave lengths $\lambda = \cdot 013a$ and $\lambda = \cdot 055a$, which form the outer square. But in addition, there are others, such as 2, 4, 2; 0, 6, 2; 4, 4, 2, which are considerably fainter and have wave lengths further from the maximum in the spectrum. The only difference between this set and the one which has $h_3 = 1$ is that all the spots are comparatively fainter and that the range of wave lengths represented is much reduced, there being now both an upper and a lower limit to the values of $\frac{\lambda}{a}$.

The same may be said of the sets of numbers which have $h_3 = 3$, there being still fewer of these; finally, only one very weak spot is visible corresponding to parameters which have

$h_3 = 4$. Below are tabulated the sets of parameters which have h_3 equal to 1, as typical of the other sets corresponding to points in the photograph.

It can be seen how complete the pattern really is, every wave length greater than $\lambda = '034a$ producing an interference maximum if there are values of h_1, h_2, h_3 to suit. One faint spot is included here which is not in the former table. The vertical and horizontal columns give the values of h_1 and h_2 respectively. In the squares are set the values of $\frac{\lambda}{a}$ and stars denoting the "magnitude" of the corresponding spot in the photograph, according to an arbitrary scale :

TABLE II-

$$h_3 = 1$$

	$h_1 = 1$	$h_1 = 3$	$h_1 = 5$	$h_1 = 7$	$h_1 = 9$
$h_2 = 1$	Off the photograph	'178 *	'073 *	'039 *	'024 Invisible
$h_2 = 3$	'178 *	'104 *	'057 *	'034 +	'022 Invisible
$h_2 = 5$	'073 *	'057 *	'039 *	'027 Invisible	
$h_2 = 7$	'039 *	'034 +	'027 Invisible		
$h_2 = 9$	'024 Invisible	'022 Invisible			

Every spot to which can be assigned a value unity of h_3 is represented in this table and it can be seen how complete the scheme is. If, on the other hand, the table had been drawn up on the assumption that when h_3 was unity h_1 and h_2 could have any integral values without the restrictions explained above, there would be either many unfilled squares or values corresponding to weak spots mixed up with those corresponding

to intense spots. Similar tables may be drawn up for the other values of h .

If the point system selected be the correct one, it is of interest to speculate what significance this has with regard to the crystal structure. This question cannot be answered definitely until results are forthcoming obtained with crystals of other systems but it is interesting to note that there is already strong evidence for associating this point system with a crystal such as zinc blende. In the first place, zinc and sulphur have the same valency and according to the theory of valency volumes of Barlow and Pope the atoms should be arranged as if they were spheres of equal volume in a system of closest packing consistent with cubic symmetry. In order that this may be so, the centres of the atoms must be arranged in this point system, centres at the cube corners and at the middle of the cube faces. Thus, if all the molecules of zinc and sulphur behaved in an identical manner towards the light waves, they would give the interference maxima actually found to exist. Again, the same point system is repeated, though on a different scale, when only those atoms are considered which are identical in every respect as regards chemical nature, their neighbours in the crystal and so forth. In the arrangement of the atoms assigned by Barlow and Pope to zinc blende and similar crystals of compounds of two atoms having the same valency, atoms of one kind are grouped together four at a time in little tetrahedra, these tetrahedra being again arranged in this point system; therefore identical atoms, one from each tetrahedron, have the desired arrangement. The element of a grating is that which, in the ideal case, repeats itself indefinitely without variation and the crystallographs give evidence of the arrangement of elements in the crystal. In a crystal such as zinc blende, it is possible to class together a certain number of atoms of zinc and sulphur in such way that the assemblage contains a specimen of zinc and sulphur atoms of all modifications and so that the whole crystal may be built up by packing together these assemblages. It is these which probably form the elements of the crystal grating; they form by repetition the crystal pattern. The simplicity of the interference pattern seems to show that it does not concern itself with the arrangement of atoms within these assemblages. Whether this is so or whether the atoms themselves are the grating elements might be settled by experi-

menting with crystals for which there is good ground to suppose that the arrangement of the individual atoms differs from that of the assemblages.

To pass to the physical aspect of the phenomena, the fact that the interference pattern is complete over a wide range of wave lengths means that the incident radiation is analogous to white light. It is not only the photograph reproduced here which supports this idea but in other cases, when the photographs which Laue obtained admit of analysis, the results conform to it. The way in which the crystal builds up from the incident radiation of all wave lengths the monochromatic trains of waves which form the spots can perhaps be best understood by considering the effect from a slightly different point of view. Since the radiation from the bulb is to contain all wave lengths, it may be regarded as a series of irregular pulses in the ether, that is to say, considered as a whole and not split up into its monochromatic components. It is in this way that Schuster treats diffraction of white light by a line grating. Pulses of this kind ought to undergo specular reflection at a plane surface in just the same way that light or heat rays do, if the plane surface differ in any way from the surrounding medium and if its "polish" be sufficiently good.

Taking advantage of the infinitely repeated pattern formed by the atoms in a crystal, it is possible to classify them arbitrarily as having their centres in sets of parallel planes. The simplest of these planes are the cleavage planes of the crystal but, of course, an infinite number of other ways are possible. When the arrangement is made in a more complicated way, the planes contain individually a very few atoms per unit area and are crowded very close together; on the other hand, the simple cleavage planes are far apart and densely packed with atoms. The "polish" of these planes is almost perfect; at any rate the irregularities due to the atom centres lying off the planes are small compared with atomic dimensions (10^{-8} cm.) and therefore these planes will be capable of reflecting waves of wave lengths 10^{-8} cm. The spots in the interference pattern are formed by the reflection of the incident beam in these planes in the crystal.

When a single pulse is reflected in one of these sets of parallel planes the atoms in any one plane only scatter a fraction of the energy in the pulse. The wavelets from all the atoms in

one plane go to build up a reflected wave front and therefore, as the incident pulse traverses the crystal, a train of reflected waves—one from each plane—is formed, the pulses in the train following each other at intervals of $2d \cos \theta$, where d is the distance between successive planes and θ the angle of incidence. These reflected pulses may be analysed by Fourier's theorem into trains of waves of wave lengths $\lambda, \frac{\lambda}{2}, \frac{\lambda}{3}, \frac{\lambda}{4}$, where $\lambda = 2d \cos \theta$.

Now though the pulses in the beam have been supposed to be quite irregular, they possess some quality which is expressed by saying that they have an average "breadth" of something like 10^{-9} cm. If in the reflected train pulses follow each other

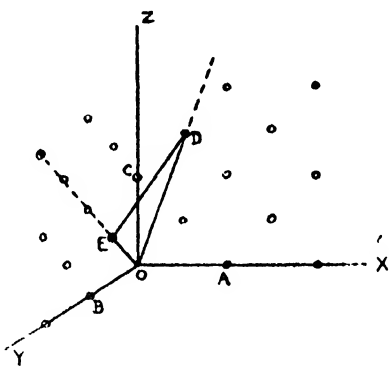


FIG. 4.

at intervals much smaller than this they will interfere and cut each other out; if, on the other hand, they follow each other at long intervals, the trains will contain little energy per unit length. Thus out of all the possible ways of dividing the crystal into planes, a certain group will be selected, these being planes for which the value $2d \cos \theta$ lies within a range of wave lengths of the order of the average "breadth" of the pulses.

This way of looking at the interference must be analytically exactly the same as that used by Laue. The numbers h_1, h_2, h_3 in his analysis correspond to parameters defining one of these methods of dividing the crystal into planes. For instance, a spot in the photograph corresponds to the values 3, 1, 1 of h_1, h_2, h_3 . This means that (see fig. 4) the wavelet from the atom at O is three wave lengths behind that from the atom at A, one wave length ahead of that from C, in the direction of the

interference maximum. Therefore, by going along a distance $\frac{1}{2}OA$ parallel to the X axis and up a distance $\frac{3}{2}OC$ parallel to the Z axis, an atom D is reached, the wavelet from which is in phase with that from O, since

$$\frac{1}{2}h_1 - \frac{3}{2}h_2 = 0$$

The wavelet from E is also in phase with that from O, since

$$\frac{1}{2}h_1 - \frac{1}{2}h_2 = 0$$

and therefore when a pulse falls on these atoms the pulses from the atoms O, D, E are all in phase in the direction of the interference maximum; that is to say, this is the direction in which the pulse would be reflected from a plane O, D, E.

The advantage of this way of looking at the formation of the spots is that it enables one to follow what happens when the crystal is not placed symmetrically. If the crystal be tilted so that the incident radiation does not pass along an axis of symmetry, the spots of the pattern will all be displaced and they will move as if they were reflections in planes fixed in the crystal and tilting with it. This is shown very well by two photographs which Laue published in his paper. The first shows the effect obtained when a trigonal axis of symmetry of the zinc blende was parallel to the incident beam (see fig. 2A); the rays now making equal angles with the three cubic axes, a pattern of threefold symmetry is the result; the spots of this pattern are reflections in planes of the crystal, some of the planes being the same as those which give the spots in the pattern of fourfold symmetry. In the second photograph, the crystal was tilted though 3° from its normal position about an axis perpendicular to one of the cube axes. The pattern is distorted exactly as it would be if the spots were reflections; it is also interesting to notice that certain spots are very much changed in intensity. If the angle of incidence of a pulse in a set of planes be altered, the value of $2d \cos \theta$ alters accordingly and so the wavelength of the reflected train may vary from a value characteristic of intense spots to one characteristic of weak ones and *vice versa*. It was found before, in the case of the square pattern, that intense spots corresponded to a wave length $\cdot 06a$ and this is true of the two photographs considered here. One spot in

particular is hardly visible in the symmetrical pattern but becomes the most intense of all when the crystal is tilted, because its wave length is now right in the maximum of the spectrum. If one imagines the crystal slowly tilted, the spots will be in motion on the photographic plate and the wave length continually changing. If only certain wave lengths were present in the incident radiation, spots would be disappearing and appearing all over the plate but as a matter of fact it can be seen from the photographs that the process is quite continuous and so all wave lengths are present.

The ellipticity of the spots can be easily understood if the pencils forming them are regarded as rays reflected in crystal planes. It is a geometrical result of the fact that the incident pencil is not strictly parallel but slightly conical. If a conical bundle of rays be reflected in a slip of crystal, at right angles to its axis, regarded as a pile of parallel plates, the rays will come to an approximate line focus on the far side at a certain distance from the crystal and the ellipticity of the spots is a result of this tendency.

It was pointed out to me by Mr. C. T. R. Wilson that if the interference phenomena could be regarded thus, it might be possible to get a strong reflection in crystals which have some very decided cleavage plane, this plane being presumably thickly packed with atoms. To test this, a narrow beam of X-rays was obtained by stops exactly as in Laue's experiments and allowed to fall on a mica plate set so that the incidence was almost glancing. A photographic plate was placed so that it would receive both the transmitted beam and the reflected beam if there were one.

It was found that in this way a well-marked reflected spot appeared after a few minutes' exposure to quite a weak bulb, whereas Friedrich and Knipping found it necessary to expose the crystal during many hours to the most intense beam of X-rays obtainable in order to get good results. The effect is almost a surface effect, quite thin plates of mica sufficing to give full reflection; it is possible that in this case the reflected radiation is less penetrating and of greater wave length than that forming the interference pattern with zinc blende. The rays are, however, little absorbed by aluminium and black paper. The reflected beam will, as before, consist of monochromatic radiations, the wave length depending on the angle of incidence

and the distance apart of the cleavage planes in the mica. No reflection has yet been obtained with an angle of incidence less than 75° but there is no reason to suppose that this means anything more than that the time of exposure was not long enough for smaller angles.

When a somewhat longer exposure (30 minutes) is given to the plate, subsidiary spots appear in a very characteristic manner. In all the crystallographs taken with crystals of any system set in any way, a certain feature of the arrangement of the spots can be traced. There are generally several series of spots forming well-marked ellipses passing through the big central spot. These ellipses are nearly circular: they are in fact sections by the photographic plate of circular cones which have the incident pencil as one generator. The reason for their appearance is as follows. The atoms of the crystal may be classed as having their centres on parallel straight lines as well as in parallel planes. Consider the crystal as divided in this way into a set of parallel rows of atoms inclined at an angle to the direction of the incident radiation. As an incident pulse passes over the successive atoms of any one row, wavelets are emitted from each atom in turn at equal intervals of time and these wavelets will be all in phase in any direction lying on a circular cone having the row of atoms as axis and the direction of the incident radiation as one generator. In all these directions at least one condition for interference is satisfied, so that the ellipse in which the circular cone cuts the photographic plate gives a locus of possible positions of interference maxima. The ellipses which are so apparent in the crystallographs correspond in this way to densely packed rows of atoms in the crystal. At the point where two ellipses intersect, two conditions for interference are satisfied; the third is satisfied by the wave length and therefore a spot is to be expected there. This effect is very apparent when the beam is reflected from a slip of mica and a somewhat long exposure given to the photographic plate. As well as the main reflected spot, there are many others reflected from subsidiary planes in the crystal. The greater number of these are arranged on two ellipses which pass through the central spot and intersect in the main reflection. They seem to correspond to a lattice arrangement of atoms in the cleavage plane of the mica, the atoms being at the intersections of two sets of parallel straight lines. The atoms are therefore in rows in these two directions,

each direction being the axis of a cone which cuts the plate in one of the ellipses.

There can be no doubt that these crystallographs must throw a great deal of light on the physical nature of these short ether waves. When an electron is shot into the anticathode of the X-ray bulb and then brought up, there must be set up those electro-magnetic pulses first supposed by Stokes to constitute X-rays. It seems very probable that the waves here dealt with are these electro-magnetic pulses and it will be of the greatest interest to discover whether they are the same as the X-rays or not. All that is known of them so far is that they are penetrating and act on a photographic plate. It is possible that there may be in the rays from an X-ray bulb two components, waves and corpuscles. The electro-magnetic pulses can be regularly reflected, can interfere, can act on a photographic plate and perhaps can be polarised. Their energy is spread uniformly over a wave front. On the other hand, the facts of the emission of characteristic secondary radiation from metals, of the equality of the speed of electrons knocked out of atoms by X-rays and the speed of the electrons which originally produced the rays in the X-ray tube, seem to be explained far more simply by supposing the existence of a corpuscular radiation. These corpuscles are represented by quanta of energy flying through space contained in a small region of invariable volume. There is perhaps the possibility of both these components having been hitherto classed together as one. This is only conjecture but at any rate it seems as if these experiments of Laue and his collaborators may solve not only problems of crystal structure but also the problem of the true nature of X-rays.

"MATHEMATICS AND CHEMISTRY": A REPLY

By JAMES RIDDICK PARTINGTON, M.Sc.

IN a recent issue of SCIENCE PROGRESS (January 1912) there is contained a very interesting discussion on the relation between mathematics and chemistry, between mathematicians and chemists and between chemists and chemists, in which the author, in addition to a criticism of the present conditions, has given us what is very much more valuable, a suggestion of what he considers to be a satisfactory method of remedying their inherent faults. Since my text-book (*Higher Mathematics for Chemical Students*: Methuen & Co., London, 1911) has been mentioned as the source of inspiration of the article and as the author says explicitly that his statements "may serve to induce discussion or criticism," I may take this opportunity of expressing my own views on what are undoubtedly matters of increasing importance, viz. the utility of a knowledge of mathematics to the chemist and the way in which he can acquire that knowledge most profitably. Although the majority of the statements made in Mr. Worley's essay are likely to meet with hearty assent from any one who approaches the subject without bias on either side, yet there are certain views expressed which appear to be highly controversial and as such call for discussion.

It would seem that the discussion must necessarily involve the answering of questions such as the following:

- (1) Is it desirable that chemists should be taught higher mathematics?
- (2) How much should they be taught?
- (3) In what way should the instruction be given?

Besides these questions of pedagogic interest, there is also the problem of the general relation between mathematics and chemistry which has received due consideration in Mr. Worley's paper. He has examined not only the relation between the two sciences but also those between their followers. From

what he says of the latter, it would appear that things have not improved very much since the time when Richter (1789) made the statement which is quoted in my book (p. 5): ". . . the most prominent chemists occupy themselves little with mathematics and the mathematicians feel that they have little business in the province of chemistry"; for we are now told that "chemists, as a rule, know very little mathematics but even when they have received what is considered to be a fair amount of mathematical training they only too frequently find that their knowledge is not sufficient to enable them to deal with the practical problems that arise; unfortunately, they also too often find that the mathematician has not sufficient chemical knowledge and feeling to give them the assistance they need." The advance probably lies in the raising of the standard of what is considered to be a fair amount of mathematical training, which is now certainly higher than that which sufficed in Richter's day.

This attitude the writer considers to be due to a real incompatibility of the chemical and mathematical habits of thought a view which is reasonable enough in itself but which leads him to what is clearly a fundamental error in natural philosophy. After saying that "it must be admitted that chemical problems are frequently of such a nature that it is impossible to be certain of anything; the chemist frequently does not know what he wants to prove nor indeed does he want to prove anything; he wants merely to put a reasonable interpretation upon certain experimental results," Mr. Worley tells us that "chemical properties are the expression of the reciprocal behaviour of substances, not absolute quantities; on this account it is very difficult to quantify such properties: often they can be felt but not figured."

The word "feeling" or "feels" occurs in fact no fewer than five times on the same page and it is quite clear that the author is referring to that medieval doctrine of the Discrimination of the Scientific Instinct which, although it should have received its death-blow when Columbus circumnavigated the earth, is apparently still very much alive. Does Mr. Worley seriously ask us to believe that it is safe to rely on our feelings when deciding a scientific problem? One example [of the results of this procedure is given in my book (p. 4); at the risk of being tedious to my readers I will add a few more. Could we reason-

ably expect a physicist to "feel" that there is a bright spot at the centre of a circular shadow, that glass is a better conductor of some kinds of electric currents than copper, that a surface of separation between two perfectly transparent media is a better reflector than polished opaque silver? Would any chemist have those "stirrings in the viscera"—as Professor James put it—which would lead him to expect that "inert" nitrogen could exist in a most active allotropic modification; or that, in spite of all that had been said about the cause of the activity of substances in a "nascent" condition, the new monatomic gases should be totally inert? We know that there were chemists who flatly contradicted the truth of the last example and that solely on the evidence of their feelings. As a last example we might take the question of the constitution of isatin, which had been settled agreeably to the feelings of chemists until Hartley and Dobbie showed that the actual facts were exactly the opposite to what we should expect. It is undoubtedly true that chemists have made instinctive guesses which have later on been shown to be quite incorrect. Some of the guesses are bound to turn out right in any case on the theory of probabilities but this is no justification for the use of guesswork as a scientific method; if scientists denied any validity to the principle of the Discrimination of the Unscientific Instinct in the time of Darwin, how can they defend their own use of an identical principle now?

When we come to deal with the three pedagogic questions, we find that Mr. Worley has spared us the trouble of discussing the first, for his paper leaves no doubt remaining that he recognises the great value, both from a practical and from an educational standpoint, which a mathematical education has for a chemist.

In dealing with the second and third problems he is less clear than could be wished. It is, of course, necessary to make up our minds at the start not only what we are going to teach but who is to be taught; the initial knowledge and the future prospects of the student must always regulate the course of any teaching that is going to be fair and straightforward, not merely the result of faddism or slavish adherence to some pet educational doctrine. I wish to make it clear at this point that I am not thinking, in referring to "the future prospects" of the student, of his ultimately competing in any

examination, for I withhold my opinion of the value or otherwise of examinations as being quite irrelevant.

The class of readers to whom my book is addressed is, I think, made sufficiently clear in its title. It is not written for experts. There is obviously a difference here which Mr. Worley unfortunately does not keep clear. After stirring up our sympathies for "the undergraduate struggling against various unnecessary and unnatural obstacles to obtain a degree," he further on leaves this unfortunate individual quite in the lurch and turns his attention to the more dignified subject of "the mathematical requirements of the chemist for the purposes of investigation and research." It must be confessed that by this sudden change of attitude the writer to a great extent robbed us of those tender feelings which he at first so successfully aroused. Although we should pity that student, what really could be our attitude towards one who had survived that iniquitous thing, "our present educational system" and was still capable not merely of "investigation" but also of "research"? He can surely be trusted to look after himself and in the rest of this paper he will be allowed to do so.

The answers to be given to the remaining questions are largely matters of opinion and can most properly be left to the judgment of the individual teacher. Since, however, Mr. Worley has given us his opinions, it may be permitted to me to express mine. The amount of mathematics which should be taught to the chemical student varies, as has already been said, with the future prospects of the latter. If he intend to devote himself to synthetic organic chemistry, he will need only very little, whereas if he be going to do original work in physical chemistry, he will require more, although still not very much as compared with the physicist. As a rough mean value, I am inclined to indicate what is set out in my text-book, which may therefore be taken as the expression of my opinion on this side. Now the more important question how the student may with the greatest advantage be taught the amount of mathematics which has previously been decided is necessary and tentatively sufficient for his requirements. Here Mr. Worley is again rather indefinite, for he says that although "chemists are taught mathematics without sufficient instructions in the way in which the weapons put into their hands are to be used and especially the way in which they are not to be used," yet "it would probably

be vastly more satisfactory if the necessary parts of mathematics were taught without attempting to deal with chemical problems, with sufficient examples and exercises to make the student proficient in the carrying out of the various processes and if afterwards real chemical problems were dealt with thoroughly." This can only be taken as meaning that the chemical student is to have the following mathematical training :

(1) A course in pure mathematics, without any indication as to what sort of use the material he is learning is afterwards likely to be to him ; and (2) another course in which the material is applied to chemical problems and in which the student is more particularly told what he is not to do with his previously acquired knowledge.

Now the first course corresponds with that which the chemical student has been accustomed to receive ; it is one of those "unnecessary and unnatural obstacles" against which he has been "struggling" and is all the less likely to be of real service for the reason that "the chemical and mathematical habits of mind are incompatible" and that "chemists as a class are never likely to be mathematicians." It is my own opinion, supported by the educational teachings of Herbart, that a mathematical process can be most readily assimilated by such persons when it is presented along with some chemical problem, just as the physical student most readily learns the Calculus of Variations in connexion with the Principle of Least Action, Fourier's series and integrals in their application to the Conduction of Heat and the theory of Probabilities as it appears in the Kinetic Theory of Gases. In a text-book of mathematics in which the aim is to teach mathematics and not chemistry, nothing can be gained by making the examples too complicated. We do not usually begin our text-books on dynamics by considering the effects of friction or elasticity ; nor in teaching the student the theory of conduction of heat do we insist on his trying his feeble strength directly on an irregularly shaped and irregularly heated heterogeneous mass cooling in draughts of air of various temperatures moving at random over its surface, *i.e.* on *real* problems. Are we then to be accused of deliberately trying to give "the impression that (physical) problems are very much simpler and straightforward than is really the case" ? I believe that Mr. Worley's accusation that I have tried to do this in connexion with chemical problems is unjust. It is true that in the book the simpler cases of mass

action are considered as well as the more complicated examples but it is also made quite clear that "there are cases in which n as derived from velocity measurements does not agree with that derived from the chemical equations" (p. 150), which apparently is what Mr. Worley is telling us on p. 410; and further on more space than usual is devoted to emphasising the uncertainty which always attaches to the determination of the "order" of an interaction by means of velocity measurements: "The view is becoming more and more pronounced that reactions of higher orders are very rare" (p. 152); "a chemical reaction is the resultant of a large number of conditioning causes . . . and therefore proceeds in a variety of ways and leads to a variety of products. It is only in a few cases that we can say exactly how a reaction proceeds in all its stages" (p. 246). I had hoped that this would not have given rise to the opinion that there was any attempt to make out that the whole matter is really simpler than is actually the case and should have thought that it would have been unnecessary for my critic to say that "the law of mass action is a generalisation of an axiomatic nature, never *apparently* obeyed exactly and incapable therefore of absolute proof; that even if the correct assumptions are made with regard to the number and nature of the interacting molecules there are many disturbing factors, as a rule, that cannot be taken into account" (p. 408). It would be interesting in the light of his statement that the law of mass action is "a generalisation of an axiomatic nature," to ask the author if he knows the difference between a generalisation and an axiom, as exemplified by the Second Law of Thermodynamics and if he has ever heard of Willard Gibb's thermodynamic demonstration of the law of mass action. After what had been said on the tendency to superficiality exhibited by existing text-books, it is not surprising to find the author stating that "it is consequently highly desirable that the mathematical treatment of a question, such, for instance, as that of mass action, should be thorough, dealing with all the difficulties that arise." One is tempted to ask if the writer has found this method possible in practical teaching?

Mr. Worley has also introduced some remarks on the theory of solution into his paper. After exciting our imagination by a moving picture in which a high edifice of "mathematical jugglery" is to be "razed to the ground," he makes us "shudder to think of the terrific downfall should the foundations give

way." We had almost begun to shudder when our fears were calmed by a sudden cessation of superlatives and the author's adding mildly, "such an occurrence is not impossible." Our peace is not of long duration, for "we may find some day that all the units of the solute are potentially active." When we ponder a little time over the phrase "potentially active," we are brought to a frame of mind in which it would cause us no surprise to find "some day" that the units of the solute were continuously distributed in discrete portions throughout the solvent, in the form of microscopic, hard, spherical, soft cubes of immense size. The author has in fact fallen in that last paragraph—perhaps by reason of some unconscious psychical process of suggestion due to the fact that the din of the last "terrific downfall" is still ringing in his ears—into a trap which one would think by this time had lost its deadliness. As this does not appear to be the case, it may not be wholly useless to repeat what has previously been said elsewhere in connexion with the subject :

"The real fundamental proposition of the thermodynamic theory of solution is contained in the assertion that the osmotic pressure of a solution and every other property conditioned solely by it, depend simply on the number of solute molecules scattered through a given volume of solution and not at all on the chemical nature of either solute or solvent or on the relation between the latter, provided only that the solution is dilute. The chemical properties of solutions, on the contrary, depend not only on the number but also on the nature of the dispersed particles and so are to a large extent conditioned by the exact mode of connexion between the solvent and solute."

"It seems necessary to emphasise this point because of the fallacy, which unfortunately appears to be widely spread, that there is some fatal incompatibility between the old qualitative hydrate theory of solution and the new quantitative thermodynamic theory of which van't Hoff was the pioneer. This view has resulted from the one-sided outlook of the champions of each theory and is certainly not a necessary consequence of the fundamental basis of either. It is greatly to be desired that writers of the theory of solution should distinguish clearly which aspect of the subject belongs properly to their own investigations and should refrain from attacking, on the basis of irrelevant experiments, a theory which is quite immune from the criticism which may reasonably be levelled against any particular hypothetical view of the nature of solutions."

HORTICULTURAL RESEARCH

II. TREE PRUNING AND MANURING

By SPENCER PICKERING, F.R.S.

IN the previous article an account was given of the results obtained at Woburn in investigations of various problems connected with the planting of trees; other experiments involving the treatment of the tree after it has been planted will be referred to in the present article.

PRUNING

In the case of trees used for ornamental purposes, correct pruning is a matter of taste and judgment, little more being required than the removal of branches which interfere either with the symmetry of the head or the shortening of branches which project too far beyond their fellows. A similar attention to symmetry is required in dealing with fruit trees but symmetry is not the only desideratum: fruit-bearing and the production of well-developed and ripened fruits should be the main object in view. This is not the place to enter into all the technicalities of the art of pruning nor is this art always amenable to investigation at an experiment station; inquiry has to be confined, at any rate in the first instance, to the main principles governing the practice of pruning.

Under the head of pruning may be included all operations which involve the use of the knife on branches or roots. It is desirable to separate branch-pruning into four categories: (1) the severe shortening of all the branches when the tree is first planted, known as cutting back; (2) the annual shortening of the new twigs formed during the season, this being what is generally meant by pruning; (3) the cutting out of badly placed branches, especially those which cross or rub against others, known as thinning; and (4) operations in summer intended to arrest growth, such as pinching off the growing tips of the twigs or half-breaking or twisting the ends of these

twigs. The removal of some of the buds from trained trees, in order to help the development of those which are left, is also, properly speaking, a form of pruning, though it is generally known as disbudding.

CUTTING BACK

The cutting off of about one-half or two-thirds of each branch of a young tree when it is transplanted from the nursery to the plantation is very generally recognised as being the proper practice, though it is often omitted by the amateur, who dislikes seeing his tree curtailed and learns too late that such parsimony is false economy. The proper functioning of a tree depends on the correct balancing of root-action to leaf-action; the one supplies the tree with water and food-material derived from the soil, whilst the other is the channel through which carbon is absorbed from the air: but as was explained in the previous article, in transplanting a tree the existing root-system is destroyed and a new root-system gradually has to be evolved: the balance between roots and branches can only be restored by curtailing the branches so as to adapt them to the injured roots. This is the *rationale* of cutting back on planting. The result of omitting the operation is very apparent, especially during the first season and is often very serious. Instead of forming good healthy leaves and a fair amount of new growth, the leaves have been found to show a deficiency of some 25 per cent. in size, little or no new wood being formed. Photographs of two apple trees which were similar when planted eighteen months previously are shown in figs. 1 and 2 (the staff shown in the figures is divided into feet); these give a fair idea of the results of the two forms of treatment. In the case of plum trees, which commonly fruit precociously after transplanting, if not cut back, the trees will often be so exhausted as to be killed.

Although good horticulturists never question the advisability of cutting back on planting, there is a considerable diversity of opinion as to when this operation should be performed, some advising that it be done at the time of planting, others at the time when growth is starting in the spring, others again advocating that it be deferred till one year after planting. A number of somewhat elaborate experiments have been made on this subject and it has been found that the time at w



FIG. 1 Control



FIG. 2 Not at full

the cutting back is performed makes no difference whatever so long as it is done before active growth sets in; trees cut back at various times between November (when they were planted) and the middle of April (when they were beginning to grow) all behaved in the same way: but when the cutting back was deferred till July, it was seriously detrimental, the trees showing a marked deficiency of growth and vitality during each of the subsequent eight years. Rather than cut a tree back in the summer, it is much better to defer the operation altogether till the following winter. The effect of such delay, however, is not good, though it varies considerably in different cases. It cannot be good for a tree to remain, even for one season, in the condition exhibited in fig 2; and even if a tree be cut back after the first year, one season's healthy growth will have been lost. In some cases a tree treated thus will continue to lag behind its fellows which were cut back on planting, whilst in other cases it has been found that very vigorous growth has followed the deferred cutting back, the tree maintaining this vigorous habit of growth for several years; the result being that it has grown only, whilst it ought to have been growing (though more moderately) and also fruiting. In one plantation of apples where this deferred cutting back had been adopted, the crop during the first five years after planting was only one-quarter of that of similar trees which had been cut back at once, though the trees themselves were 10 per cent. greater in size than the latter. In another case there was a deficiency of 40 per cent. of fruit during the first eight years.

BRANCH-PRUNING : EFFECT ON GROWTH

There are various sayings current amongst horticulturists embodying the idea that the more a tree is pruned, the more it will grow. It is obvious that whatever truth there may be in such an idea, it can only be true within certain limits; now direct experiment shows that these limits are very narrow indeed. When the branches are cut away, the roots will be in excess of the requirements of the tree and new branches will be formed, the tree endeavouring, as it were, to repair the injury. In the case of a tree which is old and has ceased to grow or of one which has become stunted from other causes,

the new wood which is thus made may rejuvenate the tree and result in a healthy growth, which would never have occurred had the old branches been left unpruned; but in the case of a tree already in a healthy condition the formation of new wood to supply the place of that which has been cut away must involve an extra tax being placed on the resources of the tree and though the tree may do more work, the results will fall short of those which would have been obtained without the pruning. In other words, a young tree which is pruned heavily every year must necessarily remain a smaller tree than one which has not been pruned.

Various plantations of different varieties of apple trees on the paradise stock have been grown side by side at Woburn under different systems of branch-treatment. The normal treatment consists of light pruning every year, involving the removal of about one-third of the length of each new shoot formed during the season; whilst in other cases the pruning is hard, two-thirds of the growth being removed; in others, again, there is no pruning. The trees are measured periodically. The results leave no doubt that the less a tree is pruned the bigger it becomes: the unpruned trees after five years showed an excess of 33 per cent. in size over the moderately pruned ones; those which had been hard pruned showed a deficit of 13 per cent. The differences naturally diminish as time goes on, at any rate in cases in which the pruning is only moderate; for after ten years the unpruned trees showed an excess of only 7 per cent. over the moderately pruned ones and after fifteen years the difference was reduced to $2\frac{1}{2}$ per cent. The deficiency in size produced by the hard pruning, however, shows no reduction; from the 13 per cent. after five years it became 18 per cent. after ten years and was again 13 per cent. after fifteen years. Figs. 3 and 4 represent average specimens of an unpruned and hard-pruned tree of Bramley's Seedling apple ten years after planting.

It is found that the deficiency in size of the hard-pruned trees is more marked as regards the height and spread than as regards the girth of stem; the former showed, after five years, a deficiency of 21 to 24 per cent. but the latter one of only 9 per cent.: this is what might naturally be expected: therefore, hard-pruning may be adopted as a means of making a tree sturdier in proportion to its size than it would otherwise have

been, though the actual thickness of the stem and branches may be less.

It might be suggested that, though a hard-pruned tree is smaller than an unpruned one, it has really made more growth, where allowance is made for the wood removed in the pruning; but this is not the case, as was proved by comparing the recorded weight of the prunings and the total weights of the trees when some of these came to be removed. This point has also been investigated in another way. Several trees were taken and on each of them a number of straight shoots of exactly the same size were selected, all 36 in. in length; some of the shoots were left unpruned, whilst others were cut back to a length of 24, 12 and 6 in. Fig. 5 shows one set of shoots at the end of the season following this pruning. It is easy to see that the harder the pruning has been the less is the growth which has taken place. On the average the unpruned shoot increased five and a half times more in weight than that which had been cut back to 6 in. and the number and length of side shoots arising from it was three times and twice as great, respectively: so that in no sense had pruning favoured growth. Shoots cut back to intermediate lengths gave intermediate values.

EFFECT ON FRUITING

These experiments also afforded evidence on another important point, though this is scarcely visible in the figures: the fruit-buds formed on the twigs were more numerous the less the pruning, so much so that there were on the unpruned twigs five and a half times as many fruit-buds as on those cut back to 6 in.

The effect of pruning in reducing the fruiting power of trees has been investigated more extensively in other experiments. In one case a record of crops was available for this purpose obtained from a ten years' trial of sixty dwarf apple trees grafted on the paradise stock of each of three different varieties. The general results for the first and second periods of five years are illustrated by the first two diagrams in fig 6, from which it will be seen that the weight of fruit obtained from the unpruned trees is about double that obtained from the moderately pruned or "normal" trees, whereas from the hard-pruned trees the yield has been but little more than half of that from these

normal trees. In another case there was a plantation consisting of eight trees of each of 117 different varieties of apples, four of each being on the crab stock and four on the paradise stock. These trees had been treated in the same way till they were seven years' old and then a difference was made in pruning them, moderate pruning being continued with one half and hard pruning adopted in the case of the rest. The results of the cropping in the following season (all the varieties did not bear fruit) are illustrated by the other diagrams in fig. 6 and bear similar evidence to that of the other experiments,

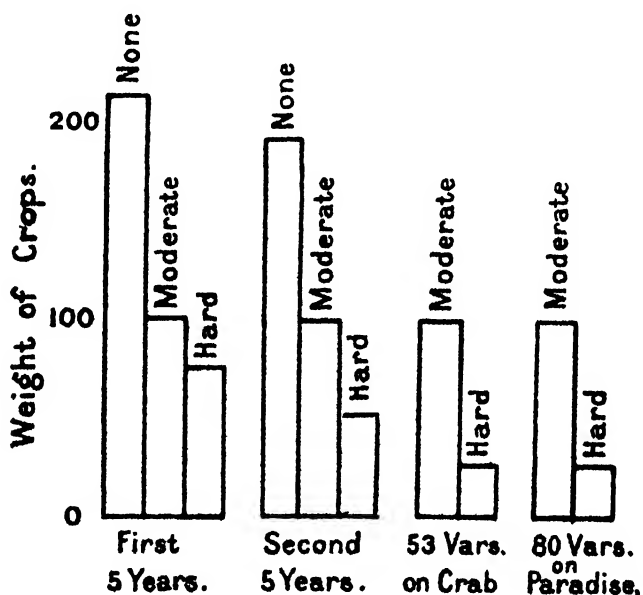


FIG. 6.—Crops from trees pruned to different extents.

the hard pruning having reduced the yield to one-third of the normal, this being equally the case whether the trees were on the paradise or crab stock. These results refer to one season only (1906) but the results have been similar in every succeeding year up to the present date (1913). Of course, some instances occur every year in which the hard-pruned trees yield the better crops but this is probably accidental, for no one variety is found to do so uniformly in successive seasons.

The way in which fruiting is favoured by an absence of pruning has received many striking illustrations at the Fruit



FIG. 3 Unpruned



FIG. 4 Hard pruned

Farm during the last few years. All the shelter hedges there, which are many hundreds of yards in length, consist of various sorts of fruit trees: these are clipped after the manner of hedges but in some cases a branch here and there has, for certain purposes, been left uncut; these uncut branches, especially in the case of plums and damsons, are always loaded with fruit, whilst the whole of the rest of the hedge is often quite bare.

One point which has caused us some surprise is that the increase in crop produced by absence of pruning has not been accompanied by any serious reduction in the size of the fruit. Thus, taking the ten years' results with dwarf apple trees previously quoted, the average size of the fruits from the unpruned trees was only 4 per cent. less than that of the fruit from the moderately pruned ones; that from the hard-pruned trees being 18 per cent. greater. These differences would not compensate for the much greater differences in the actual weight of the crops, so what has been said as to the effect of pruning on the weight of fruit obtained applies with almost equal force to the value of that fruit.

PRACTICAL APPLICATION OF THE RESULTS

It is thus established as a fundamental principle that the less pruning there is the more will the tree grow and the more fruit will it bear. There are, however, considerations which render it advisable in practice not to dispense with pruning altogether. While a young tree is growing the chief object of the grower should be to condition its growth in such a way that when it comes into bearing it should be able to carry its crop to the greatest advantage: the branches should be evenly disposed and should be far enough apart to admit light and air to the centre of the tree; none of these should cross or rub against another and they should be stout enough to bear any reasonable weight of fruit without being bent out of shape or broken. To attain this end some pruning will be necessary, for, as has been mentioned above, one effect of pruning is to make a tree comparatively sturdier; a branch will occasionally have to be removed altogether, whilst others must be pruned hard so as to restrict their extension in length until they have become stout and strong. This will generally mean a certain but decreasing amount of pruning for five or six years after the tree

has been planted, when the annual pruning may be reduced to the removal of a few terminal inches of the twigs, which generally consist of wood which has not ripened and would probably give rise to feeble growth in the following year. Combining these considerations with the general principle mentioned above, we should say that, after the first cutting back of the tree, pruning should be restricted to such an amount as is necessary for the formation of good sturdy head to the tree.

What extent of pruning will be necessary to effect this will vary very much with the nature of the particular tree. A variety which is a strong grower and a shy bearer in its early years will require very little pruning. The tree shown in fig. 3 is a variety of this sort (Bramley's Seedling). Although in this particular instance no pruning whatever has been done, the tree is fairly well formed and sturdy, being capable of carrying a large crop of fruit : to a considerable extent it has pruned itself, just as most forest trees do, branches originating and flourishing only where they are wanted, that is, where there is enough light and room for their free development. On the other hand, fig. 7 illustrates the result of not pruning a tree which is a less sturdy grower (Stirling Castle) and bears heavy crops even when quite young. The branches, as will be seen, are all bent out of shape and when laden with fruit, much of this will be destroyed by being on the ground or by being whipped off by the wind.

Instances of the harm done by the absence of pruning when a tree is young may be seen in nearly any farm orchard throughout the country and even in the plantations of growers in most of the fruit-growing districts. But examples of over-pruning are, perhaps, not less frequent and are generally to be found in private gardens, where the stunted trees throw out every year small forests of thin twigs, which are as regularly removed and only serve the purpose of feeding a bonfire.

PRUNING AT DIFFERENT TIMES

It is often held that pruning should be done in the autumn and that injury or loss of sap is likely to occur if it is done in very cold weather. Such views appear to be ill-founded. Pruning has been done in all states of the weather at Woburn and no injury has ever been noticed, even in the severest frost. One of the plantations there—a mixed plantation of a quarter

of an acre—was divided into three equal sections, one being regularly pruned early in the autumn, the other in mid-winter, the third in the spring; during the eight years throughout which the records extended, the three sections showed no appreciable difference, the total values of the crops being in the proportion of 109 : 94 : 100.

SUMMER PRUNING

The results obtained at Woburn on summer pruning are not yet complete and are still somewhat ambiguous. So far, the performance of the ordinary annual pruning in summer instead of in winter has led to no appreciable alteration in the behaviour of the trees, either as regards their growth or their fruiting. But under the general term "summer pruning" are included other operations which fall short of actual pruning and are generally followed by ordinary pruning at the end of the season. These operations consist of anything which will check the growth of the twigs and lead to the swelling of the buds lower down on the stems. Sometimes the ends of the shoots are pinched off; or the shoots may be partially broken and left hanging on the trees; or they may be twisted between the thumb and fingers, so as to be injured. In many cases, no doubt, the buds below the point of injury receive, in consequence, a larger supply of nourishment than they would otherwise do and they become converted into fruit-buds for the following season. Such a result, however, is somewhat uncertain and is dependent on the character of the weather following the operation; for if this favour vigorous growth, the buds which should only have swelled will be forced into activity and the result will be a mass of summer growth, consisting of short twigs which will not ripen properly and will have to be cut away in the winter.

INFLUENCE OF THE AGE OF THE TREE

All that has been said above as regards pruning applies to trees which are still in what may be termed the growing stage and in some respects will have to be modified when it is a question of older trees. This is so as regards the principle that we get more growth according as the pruning is more restricted. It must be recognised that with trees, as with animals, there are certain periods in their life-history which

are characterised by certain differences in behaviour. There is, first, a period of rapid growth, when, in the case of a tree, branch-formation is prominent, corresponding with the increase of stature in the case of animals. In the second period branch-formation becomes insignificant ; the tree has attained its limit of size and such wood-formation as occurs goes to increase the substance of the stem and branches already in existence. This is the period of full bearing. Such new shoots as are formed at the time are rather fruiting twigs than future branches ; the general outline of such a tree will remain practically unaltered during twenty or thirty years. It is only when a branch is removed that anything approaching to branch-formation will occur, the tree endeavouring, as it were, to repair the damage done. The pruning of a tree at this stage, therefore, will result in the formation of a greater length of new wood than would otherwise have occurred. As an instance may be quoted the results with two similar fifteen-year-old apple trees on the paradise stock : the one which was not pruned formed twigs totalling 2,200 in. in length during the season, whilst the other, which was pruned, gave growth to twigs measuring 6,700 in.

It may be added that a third period in the life of a tree may be recognised—that of senile decay—which is generally characterised by a strenuous attempt to reproduce its species before death, as evidenced by the bearing of heavy crops of small fruit—worthless, however, from the point of view of the grower—and by sending up of numerous suckers from the roots.

METHOD OF CUTTING THE BRANCHES

Of the technical details of pruning very little need be said here. It is generally held to be of importance to prune back to a bud which is pointing in the direction in which it is desired that growth may extend ; in most cases this will be a bud pointing outwards, so that the branches may spread apart from each other, though some varieties of apples are so straggling in their habit that the reverse is desirable. That the position of the bud influences the direction of the growth arising from it is, no doubt, true, though perhaps not to such an extent as is generally supposed, for it is sometimes found impossible to recognise the difference between similar trees which have been pruned for many years to inside or outside

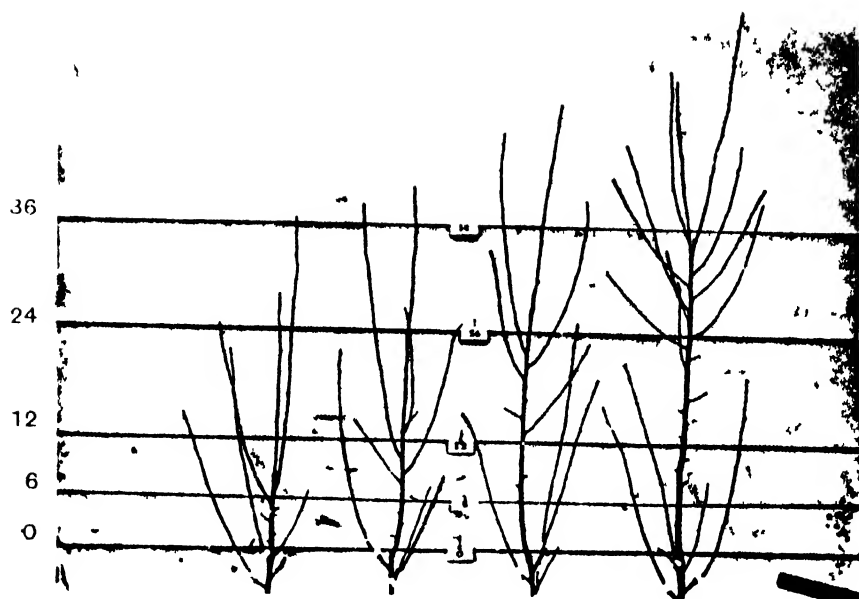


Fig. 5



Fig. 7 *Uprunellus laevis* (Laevis)

buds. In cases in which a difference has been made it was found, also, that the trees pruned to the inside buds made the greatest growth, due, no doubt, to the branches being closer together and, therefore, getting more drawn up. Other details which are insisted on—that the cut should be a slanting one and as near a bud as possible—seem to be quite unimportant and to make no difference to the well-being of the tree: when a branch is cut a callus always forms at a bud and in a plane at right angles to the branch; any wood above it dies and is cut off from communication with the living wood below by the callus. These dead snags may be unsightly but they are not detrimental to the tree; in our experiments on the subject, trees pruned even two inches above a bud have always done better than those pruned in the orthodox way, because, no doubt, the bud is weakened by having the wood cut away close to it.

ROOT-PRUNING

Wood-formation and fruiting are to a certain extent antagonistic to each other: a tree which is growing vigorously will be too much exhausted in the process to bear heavily; by putting a check on the growth, the cropping may be increased. One method of doing this is by root-pruning. The tree, if young, may be lifted bodily and the roots shortened; or if older, a trench may be dug down around it and all or some of the roots pruned. The check thus given to a tree is a serious one. In some plots of dwarf apple trees on paradise stock root-pruning has been practised regularly since the trees were planted. In one case this was done every fourth year, with the result that, after fifteen years, the size of these trees was only 75 per cent. of that of similar trees which had not been root-pruned; in a second case the trees were root-pruned every other year and their size was reduced to 35 per cent. of the unpruned trees; whilst in a third case they were root-pruned every year: these trees did not grow at all and after about fifteen years were all dead. In the case of the least severe treatment (pruned every fourth year), the trees bore heavily, principally in the second year after the pruning; but owing to the reduction in size of the trees, the actual amount of fruit borne was, on the average, only 44 per cent. of that from similar trees which had not been root-pruned. Where the

root-pruning was more frequent the total weight of fruit was quite insignificant.

Evidently root-pruning is not an operation to be indulged in except in extreme cases and then only sparingly, when, for instance, a tree persists in making rampant growth and does not flower. (The absence of fruit, be it noted, if the tree has flowered, is not a case demanding root-pruning; it is generally due to the flowers not having been properly fertilised.) Root-pruning is rarely indulged in except in private gardens; in nine cases out of ten its practice there is due to excessive branch-pruning. The effect of the latter is, as has been seen, to reduce the fruiting, hence the necessity of pruning the roots in order to restore the balance between roots and branches. But it is not a very rational method of treating a tree to injure it in one way and then injure it in another to counterbalance the damage done. If there was less branch-pruning we should hear very little about root-pruning. In one general case only may it be inevitable, that of strong-growing trees planted against a wall; severe branch-pruning is necessary, if the tree is to be confined to the wall and this will entail a corresponding pruning of the roots.

MANURING

The most conspicuous features of the results obtained at the Woburn Experimental Fruit Farm on the subject of the manuring of fruit trees is the smallness of the effect produced by any manures on apple and similar trees and the essential difference between the requirements of these and of the smaller fruits, such as gooseberries, currants and strawberries. Doubtless these results, as must be the case with all manurial experiments, are largely dependent on the nature of the soil but it must be borne in mind that the soil of the farm is by no means of exceptional richness, as measured either by analysis or by the behaviour of farm crops in it before it was converted into a fruit farm. It was nothing more than agricultural land of moderate fertility, which would probably be below the average as a favourable soil for fruit growing; the upper layer of good soil is only about seven inches deep and below that there is a very stiff clay subsoil, into which the roots of trees show a great disinclination to penetrate and from which, therefore, they

can derive very little nourishment. Yet, in spite of this, manure has had no effect on the trees.

There are twenty-one plots of dwarf apple trees devoted to these experiments ; each contained originally eighteen trees but the number has now been considerably reduced. They may be divided into three groups : one lot receives a normal dressing of manure, either artificial or natural, this normal dressing consisting of twelve tons of stable manure to the acre or a mixed chemical manure, probably equivalent thereto ; the second group receives less than the normal or no manure at all ; the third, more than the normal, up to ten times the ordinary amount of artificials or two and a half times the ordinary amount of dung : some of the plots receive artificials as well as dung. These dressings have been applied every year since 1895.

Taking the records for the first ten years, during which the plots contained their full complement of eighteen trees each, those receiving extra manure prove to be only 3·7 per cent. ahead of those receiving the normal amount ; the plots receiving a deficit are also ahead of the latter but to the extent of only 0·7 per cent. These values apply to the combined results of annual measurements of the leaf-size, triennial measurements of the trees and annual records of the value of the crops. Each of these sets of data gave very similar results. The average difference of about 3 per cent. between the groups of plots under the extreme differences of treatment is so small that it may well be attributed to error. The observations have now been continued with part of the trees during another seven years and the average differences of these later records are even less than those quoted above.

These results have been put to the test in two different ways. One lot of trees embraced in the experiments was removed after ten years but the manurial treatment of plots was continued and farm crops were grown on them—potatoes for two years and onions for one year ; and it was found that on these crops the manures had the ordinary effect which they have in other soils ; for instance, in one of the seasons the value of the potatoes in the plots with excess of manure showed an excess of 70 per cent. and that in the plots with deficit of manure a deficit of 9 per cent., as compared with those receiving moderate dressings. In the second place, the experiments with apple trees were repeated in a very poor sandy soil and here

the effect of manuring was very considerable, showing that the method of experimentation was not in fault: the influence of the treatment on the growth and fruiting of the trees began to be appreciable after the first or second season and has become more marked as time went on: thus in the seventh year after planting the value of the crops from the trees receiving a deficiency of manure was 40 per cent. below the normal and that from those receiving extra manure was 30 per cent. above the normal.

Whether or not manures will eventually have an effect on the trees at the Fruit Farm is a matter of conjecture; it is certain that they have been quite unaffected by all that has been applied to them during the past seventeen years. 'No sweeping conclusion can, of course, be drawn from this that such trees should never be manured; but inasmuch as our field does not appear to be exceptional, it seems certain that trees would frequently exhibit a similar behaviour elsewhere and that a grower would be wise before spending money in manure to make sure, either by experiments on a small scale or by considering the results obtained by his neighbours, whether that manure is likely to be beneficial in his own case; otherwise, as with us, all his expenditure in dressing his land will be wasted.

Whilst manures have been thus ineffective on apple trees, it is remarkable that, in this same soil, they have proved to be absolutely essential to bush fruits. Thus with gooseberries, plots containing four plants of each of forty-five different varieties have received continuously different dressings. During the first five years the crops from those receiving dung were 35 per cent. greater than those receiving no manure and the superiority in the size of the fruits was very marked; but artificial manures had very little effect, at any rate, on the cropping, the average yield from plots so treated being only 1 per cent. above that from the unmanured plots. These experiments have been continued for fifteen years and the plots now are even more striking than they were at first; for whilst those which have received dung have 37 per cent. of the original bushes planted in them still alive, the unmanured plots have only 9 per cent. and those receiving artificials 23 per cent. The fruit is now quite valueless except from the bushes receiving dung.

The effect of varying the amount of the dressings was

interesting. Taking the first period of five years, an increase in the dung from 12 to 30 tons per acre had no effect in increasing the crops but it increased the growth considerably; with the 12 tons this growth was six times that in the unmanured plot but with 30 tons it was ten times this quantity. In subsequent years, however, this increased growth in the early stages told on the cropping and the crops from the heavily dressed bushes are now, after fifteen years, double to treble those from the lightly dressed ones.

The effect of artificial manures on growth was similar to that of dung but much less marked. When these artificials were equivalent in supposed manurial value to the 30 tons of dung, the growth was about 80 per cent. more than in the unmanured plot but with artificials equivalent to only 12 tons of dung no increase in growth was obtained.

Thus dung is essential to the well-being of gooseberries in our soil and probably in all soils; the same has been found to be the case with black and red currants and with raspberries. With strawberries the results have been somewhat different, for though they were benefited considerably by manuring, the superiority of dung over artificials was not marked and in some seasons was in favour of the one, in others of the other; but as regards the size of the fruits, there was a distinct balance in favour of the dung.

The very different manurial requirements of fruit-trees and bushes render it evident that, to obtain the most economical results from this point of view, they should be grown separately and not in mixed plantations; other considerations, such as economy of space, may often, however, necessitate modifications of such an arrangement.

MEASUREMENT OF RESULTS

The problem as to the measurement of results in the case of fruit-trees is by no means simple and was one of the first which had to be attacked at Woburn. From a fruit grower's point of view it is clear that the fruit borne should be the criterion; but it would have to be the fruit borne during the whole life of the tree; as that may extend to fifty years or more, such a method of measurement is hardly practicable. The annual crops, it is true, must always be recorded, not only

the total weight of the crops and the average size of the fruits but these data must be supplemented with others less dependent on climatic peculiarities and the chance depredations of insects, etc. Measurements of growth must be made and growth is the most important function of a young year, for the more it grows, the larger will be the crops that it will be able to bear when it comes to maturity. In experiments which are designed to last only three or four years, the total increase in weight of the tree may be determined; for this purpose, as well as to ensure uniformity, the trees are always weighed before planting. When the experiment has to continue for a longer time, other methods must be adopted. One of these is to determine at intervals the general size of the trees by measuring their height, the spread of the branches and the girth of their stems. Another is to measure the total length of new wood formed during the season, this being supplemented by weighing the prunings. A third depends on determining the relative size of the leaves: the sixth leaf from the end of each shoot is removed and these leaves are dried and weighed. Occasionally where the trees are small, the whole of the leaves are removed and their weight determined.

The results obtained by these various methods have been compared with each other, as well as with determinations of the dry matter and nitrogen in the leaves; it is satisfactory to find that they all show a substantial agreement: thus in each of eight experiments eight different methods of measurement were adopted and the order in which the experiment could be arranged according to one of the methods was the same as that obtaining in the case of the other seven, with only two partial exceptions. Naturally, the actual magnitude of the differences when measured by different features is not the same, for some features will be more affected than others by different treatment, *e.g.* the length of new wood formed varies through greater limits than the size of the leaf. Where crops have to be considered complication arises, for growth and cropping are antagonistic to each other; and such cases always call for special discussion.

THE DISCUSSION ON ANIMAL NUTRITION AT DUNDEE

RECORDED BY F. J. RUSSELL, D.Sc.

THE new agricultural section of the British Association has adopted the useful rule of holding at each meeting a discussion on some important agricultural problem of local as well as general interest. Animal nutrition was selected as the subject for Dundee and the section was fortunate in being able to bring together physiologists, agricultural chemists and practical feeders, so that each party could present its particular point of view for the consideration of the others. Unfortunately the discussion on the origin of life somewhat interfered with the attendance.

There can be no question as to the value of the discussion. The problem has long been under investigation and each of the three groups of workers had a considerable fund of established fact to draw from. In general too, facts and data familiar to one group were new to the others, so that in the conversations that arose after the meeting it not infrequently happened that a communication one group thought was lacking in originality another group considered new and interesting. The interest displayed in the discussion was real and spontaneous, as indeed is almost always the case when a subject has a human or practical side. But of chief importance was the fact that men who are very differently occupied were brought together and that a genuine attempt was made by all to extend their several mental horizons. Agriculturists who have remained faithful to the traditions handed down by an older school of physiologists and have accumulated a large body of data on the nutritive values of different foods were able to assure the physiologists that none of the present methods of evaluating foods gave results entirely in accordance with the facts.

The physiologists had irrefutable evidence to offer that no single scheme can completely express the value of foodstuffs: neither the protein minimum and energy value nor the starch

equivalent nor any other method affording more than an approximation to the truth. Great stress was laid on the subtle principles now considered essential to nutrition; in fact, the meeting was fast drifting into the position that all nutrition is a matter of subtle principles when it was sharply pulled up by Dr. Crowther, who delivered a spirited defence of starch equivalents. Dr. Crowther declined to break off with the old love till he knew more of the new and emphasised the marked services rendered to agricultural chemistry by the admittedly imperfect methods now on their trial. The agricultural chemist is under the daily necessity of advising farmers as to the purchase of feeding stuffs and it is futile to condemn methods which do work in a way until new methods are forthcoming. The position finally reached was that the nutrition of an animal depends not only on the supply of carbohydrates, fats, proteins, etc., of which the agricultural chemist already takes cognisance but also on certain subtle compounds wanted probably only in small quantity; furthermore, that the molecular structure of the compounds wanted in large quantity (*e.g.* the proteins) must be considered. Although this perhaps represented no very great advance, it was satisfactory to find that there was so close an agreement between the views held by the physiologist, the agricultural chemist and the practical farmer. It was still more satisfactory to agricultural chemists to find that difficulties which had arisen in the course of their animal nutrition work are already under consideration by physiologists and apparently in a fair way to being solved.

Such is a general impression of the result of discussion. Before passing to the remarks of the various speakers, it may be pointed out that the practical farmer long ago learnt how to fatten animals and that he has a store of empirical knowledge on the subject. Great stress is laid on regularity of meals, quietness, etc.; it is noteworthy indeed, as was remarked at the meeting, that the details of the methods of fattening bullocks given by one very successful farmer were surprisingly similar to those adopted for human beings in sanatoria. Thus the animals are regularly turned out at the same hour each morning, fed with weighed quantities of food at regular intervals, cleaned up and bedded for the night at a definite hour; and each one is kept under close observation.

The main difficulty in conducting experiments on the nutri-

tion of animals arises from the necessity of working with large numbers; it is this circumstance that gives peculiar value to the experimental work done by Mr. William Bruce, of the Edinburgh and East of Scotland Agricultural College, whose communication was the first taken.

THE VERDICT OF THE BULLOCK

(WILLIAM BRUCE)

The experiments to which this communication relates were designed to test feeding stuffs and rations as used under the ordinary conditions of farm practice. The object in view was to provide practical guidance for the farmer rather than to deal with any scientific questions with regard to animal nutrition. Nevertheless, at least one point has emerged that is closely connected with this subject.

It may be noted here that a special feature of these experiments is the scale on which they have been carried out. With the object of eliminating individual variation and reducing the probable experimental error to a minimum, larger lots of animals were employed than is usual in such work. Besides this, some of the findings have been checked and confirmed by repeating the trials.

As the experiments extend over eight seasons (1904-12), it is impossible on the present occasion to discuss all the conclusions arrived at. Two issues which are of both practical and scientific interest have therefore been singled out for discussion. These are:

(1) The bearing of some of the results on the "starch equivalent" method for the valuation of feeding stuffs.

(2) A comparison of the value of the feeding stuffs as determined by the experiments.

The "starch equivalent" method of valuing a feeding stuff consists in analysing the material under consideration and multiplying the analytical results by digestibility co-efficients which have been determined by digestion experiments with the foodstuff in question. The figures so obtained for the several digestible nutrients are then multiplied by their respective energy values, starch being taken as unity.

The special point of the method lies in the attempt that is then made to deduct from this total energy value a figure

representing the amount of energy required for the digestion of the nutrient material over and above that which would have been required had the nutrient been starch. The energy value so deducted is supposed to be that necessary for dealing with foodstuffs in which the percentage of fibre is considerable and therefore the figure deducted represents the percentage of fibre multiplied by a factor which varies from 0.29 to 0.58 according to the character of the foodstuff. The values of feeding stuffs are thus reduced to starch values or equivalents and expressed in numbers that should indicate their relative value.

The method has been accepted generally as being by far the best of the chemical methods that have been proposed for valuing feeding stuffs and one naturally does not elect to criticise it in any hostile spirit but rather the opposite. It remains, however, to be seen how far it will apply to the practice of feeding.

The first point of interest that was observed on studying the results of the East of Scotland experiments from the standpoint of starch equivalents was the fact that in a comparison of Bombay and Egyptian cotton cakes, the former, although the poorer of the two on analysis, gave consistently somewhat better results.

Between 1903-6, the value of Bombay cotton cake was very thoroughly tested in three series of experiments which were conducted in East Lothian in the winter feeding of half-bred (Border-Leicester \times Cheviot) hoggets. In each of the three seasons the trials were carried out with six lots of sheep; in the first, the lots contained thirty-eight animals; in the second and third twenty-two and thirty-two respectively were used. They were folded on turnip land and each lot got as much food as it would consume, subject to certain limitations as to the quantity of the several items composing the respective rations. The feeding began in December and was continued during three to four months.

In the second season, Egyptian cotton cake was compared with Bombay cotton cake. The details of this particular part of the experiment are as follows:

	Egyptian cotton cake.	Bombay cotton cake.
Total roots consumed	255½ cwt.	267½ cwt.
„ hay „	818 lb.	558 lb.
„ concentrated food consumed	1,697 „	1,699 „
„ live weight increase	760 „	830 „
Increase per head per week	2.325 „	2.539 „

The composition of the cakes was as follows :

	Albuminoids.	Oil.	Carbohydrates.	Fibre.
Egyptian cake (per cent.) .	20'9	5'1	32'2	23'8
Bombay " " " .	19'0	5'4	35'0	22'3

It will be observed that the total quantities of food consumed in this case are not the same and those acquainted with the analysis of cotton cakes will also notice that the Bombay cotton cake was rather above the average in composition. But giving due weight to these two factors, the results were a striking departure from what might have been anticipated from a comparison of the starch equivalents of the two rations.

In the following season an experiment in cattle feeding was conducted in which two lots, each composed of eight carefully selected two-year-old fattening bullocks, were fed alike in every respect except that one received Bombay cotton cake and the other the same amount of Egyptian cotton cake. The analyses of the two cakes used were as follows :

	Albuminoids.	Oil.	Carbohydrates.	Fibre.
Egyptian cake (per cent.) .	22'5	4'9	32'9	21'5
Bombay " " " .	18'6	3'4	35'2	22'2

The result of this experiment was that during equal periods both lots made the same live weight increase, namely 290'5 lb. per head or 2'07 lb. per head per day. Thus the Bombay cotton cake, although shown by analysis to be a somewhat poor sample, gave results equal to that obtained with the richer Egyptian cotton cake.

These two experiments proved the value of Bombay cotton cake as a feeding stuff and pretty clearly indicated that per unit of nutriment it is more valuable than Egyptian cotton cake.

Turning to a series of experiments undertaken in 1911-12, for the purpose of comparing coconut cake and wheat bran with linseed cake as foods for fattening cattle, we get a much more definite case of departure from what might be anticipated from the starch equivalent values. Three lots of fourteen bullocks of about 1,000 lb. weight were fed in all respects alike except that one got 4 lb. linseed cake, another 4 lb. coconut cake and the third 4½ lb. wheat bran per head per day. The common basal ration was 90 lb. swedes, 12 lb. oat straw and 4 lb. Bombay cotton cake. The trials lasted 112 days: the

quantities of the foods under trial and the results obtained were briefly as follows :

	Lot I. Linseed cake.	Lot II. Coconut cake.	Lot III. Bran.
Total quantity	6,278 lb.	6,278 lb.	7,420 lb.
Starch equivalent (per cent.) .	72.35	79.31	42.76
Total starch equivalent	4,542	4,978	3,172
„ increase (14 cattle)	3,522 lb.	3,087 lb.	3,172 lb.
Increase per head per day . . .	2.27 „	1.91 „	2.02 „

These figures are so remarkable that with the object of ascertaining their suitability for comparison they have been subjected to careful examination.

Scrutiny of the increase shows that the individual increases are quite as good as can be expected. There are no notable deviations. It may be mentioned that the cattle were fed as six lots, each experiment being thus carried out in duplicate and the fact that the results of the two series agree remarkably well is evidence of trustworthiness. The probable error of the gain has been calculated from Wood's figure of 14 per cent. as the probable error of a single animal and are given below. The actual probable error in these experiments appears to be in the neighbourhood of 11 or 12 per cent. for a single animal, so that the probable errors of the amounts gained are at least not greater than those given. It may be noted that they are small compared with the difference in the average daily gain. They indicate for instance a 15 to 1 chance that the daily gain of the linseed cake lot was at least 10 per cent. greater than that of the coconut cake lot.

	Lot I. Linseed cake.	Lot II. Coconut cake.	Lot III. Wheat bran.
Starch value of basal ration . . .	10.35	10.35	10.35
„ „ additional ration	2.90	3.18	2.02
Daily gain (lb.)	2.27 ± .085	1.91 ± .07	2.02 ± .075

The total starch values and the relative efficiencies of the three rations are :

	Lot I. Linseed cake.	Lot II. Coconut cake.	Lot III. Wheat bran.
Starch value of total daily ration	13.25	13.53	12.37
Assumed necessary for maintenance . . .	6.0	6.0	6.0
Starch value available for live weight increase .	7.25	7.53	6.37
Do. as percentage of ration No. 1	100	103.9	87.9
Average daily gain as percentage of Lot I. .	100	84.1	89.0

In order to arrive at something which will represent approximately the starch value available for gain, the figure representing

the starch equivalent considered by Kellner to be necessary for the maintenance of the cattle of the size used has been deducted. This leaves a figure which represents the starch equivalent of the ration which is available for maintenance in each case and one would anticipate that the daily gain would be in proportion to this figure. If this were so the coconut cake lot should have made about 4 per cent. greater gain than the linseed cake lot and the bran lot about 88 per cent. of the gain of the linseed cake lot. Actually the bran lot made 89 per cent. of the gain of the linseed cake lot, which must be regarded as extremely close agreement with expectation; but the coconut cake lot made 16 per cent. less gain than the linseed cake lot.

The second point advanced for discussion is a means of establishing a relationship between commercial values of different feeding stuffs. Most experiments stop short at determining the relative value of feeding stuffs at the prices current when the experiment is conducted; consequently there is some difficulty in applying the results, because the market prices of feeding stuffs fluctuate and accordingly change in relation to each other. The chief difficulty arises from the fact that the price of a feeding stuff has to cover two things of importance to the farmer, namely the consuming value and the manurial value. An attempt to deal with this difficulty may be given in concrete form. In seasons 1909-10 and 1910-11, a series of cattle-feeding experiments was undertaken to determine the value of soya bean cake by comparing it with linseed cake in the winter feeding of cattle. A number of lots of cattle consisting altogether of seventy-two animals were equally divided and fed exactly alike in all respects except that the one half got 4 lb. of linseed cake per head per day and the other received 4 lb. soya bean cake. In this way 6 tons 18½ cwt. of the two cakes were consumed. The increases were as follows:

	Live Weight increase.			Cost per cwt.	
	cwt.	qr.	lb.	s.	d.
Linseed cake	84	1	24	37	8½
Soya bean cake	78	0	5	35	5½

The difference in cost thus amounted to 2s. 3d. per cwt. live weight increase in favour of the soya bean cake or 25s. 3d. per ton of that food consumed.

The linseed cake cost £9 5s. per ton and the soya bean cake £6 15s.; when the value of their manurial residues, namely

41s. and 52s., are deducted, the net food costs are 144s. and 83s. respectively. But according to the results of the experiments, the soya bean cake is worth 25s. more and therefore its relative food value becomes 108s. when the food value of linseed cake is 144s. Thus the food value of soya bean cake was three-fourths that of the linseed cake and its purchase value, taking linseed cake at £9 5s., would be $\frac{3}{4}(185s. - 41s.) + 52s. = £8$ per ton.

If the results of the coconut cake, bran and linseed cake experiments already described are dealt with in the same way, it will be found that the consuming value of both coconut cake and bran is 62·6 per cent. that of linseed cake.

These experiments, so far as they go, indicate that composition and energy value are not the only things to be taken into account in feeding. It appears that certain foods either have a peculiar feeding value apart from that indicated by their composition or that certain substances combine to make a good ration and other substances do not.

THE DISCREPANCY BETWEEN THE RESULTS ACTUALLY OBTAINED AND THOSE EXPECTED FROM CHEMICAL ANALYSIS

(DR. F. GOWLAND HOPKINS)

It seems characteristic of the present moment in science that fundamental conceptions, which we had looked upon as established, concerning which our teaching had become dogmatic, should prove to need revision.

The science of animal nutrition, though no one has pretended that, in any of its departments, the data are exact, has certainly developed its own quota of dogma.

We have long taught, for instance, that satisfactory criteria of the efficiency of a dietary (assuming the presence of the necessary inorganic constituents) are furnished by its content of protein and energy considered solely from the quantitative standpoint. A dietary, to be efficient for this or that animal, we have taught, must contain a certain, rather vaguely known, minimum of protein and a more exactly determined minimum of total energy. We have commonly been content to evaluate the protein by multiplying estimated nitrogen values by a numerical factor; the energy from calculations based upon calorimetric

determination carried out with pure proteins, carbohydrates and fats.

These data, which refer to the diet as raw material, must (we have recognised) be qualified by determination of such variants as digestibility, absorbability and the like; but the amounts of "available" protein and of "available" energy have remained our sole essential criteria of efficiency in diets. That all the assumptions implied in this limitation are become dogma is seen when we read the latest writings of the highest authorities.

Yet observations made during quite recent years (and I feel that those just detailed for us come into the category) show that our criteria and definitions have been incomplete. The food supply of an animal may, as a matter of fact, contain protein in sufficient amount, also abundant energy and yet may support the animal inefficiently or fail altogether to support it: this, too, when, to the best of our knowledge, the inorganic supply is correctly adjusted.

We have learnt that the efficiency of the protein supply is not to be defined by its amount alone. Ten or twelve years' work upon the chemistry of "protein" carried out by Emil Fischer and his school, as well as by others, has made it abundantly clear that the term covers a multitude of substances which, however closely related, differ so considerably that they must have different nutritive values for the animal body. We must for the future define an efficient protein supply in terms of quality as well as quantity.

The nitrogen-free constituents of food we have been prone to consider as sources of energy alone, as so much fuel. Since Rubner has shown that fat and carbohydrate burn isodynamically in the body, so that the place of a certain amount of carbohydrate in a dietary can be supplied by a quantity of fat containing its equivalent in energy, without affecting the metabolic balance of the animal, we have troubled ourselves but little about the relative amounts of carbohydrate and fat present in a food mixture. Questions of convenience, digestibility and the taste of the animal have, of course, intervened to determine this ratio in practical cases; but we have looked upon the total energy as the one really essential factor. Yet recent observations have proved abundantly that once an animal is totally deprived of carbohydrate, no matter how much

energy is at its disposal in the form of protein and fat, its normal metabolism is undermined ; fats are incompletely burned, all stability of protein metabolism disappears and health fails. Carbohydrate, like protein, serves other purposes than that of mere fuel and a minimum of the former is as necessary as a minimum of the latter. The isodynamic law of Rubner holds within limits only: carbohydrates and fats are not, *au fond*, physiologically equivalent. One does not know how far this fact may prove to have practical importance. Practical dietaries probably all contain the necessary minimum of carbohydrate; but it is well to point out that for individual species an optimum amount may exist not identical with the minimum. About this we know nothing. It may quite well prove worth while to determine more exactly the effect upon nutrition of altering the carbohydrate : fat ratio during prolonged periods.

Apart from considerations relating to the better known constituents of foods, we know from the work of the past year or two that quite unsuspected factors are essential to the normality of diet. An absence from the animal's diet of substances to which it is accustomed in very small amount may produce startling results. Feed a man on intact rice grains and he does well. Supply him with decorticated polished rice alone and he develops disease of the severest type. Restore a substance present in very minute amount in the cortex of the grain and you restore nutritive power to the polished grain. Feed a young animal on an artificial mixture of pure protein, fat, carbohydrate and salts and it ceases to grow, even when the amount consumed is quantitatively adequate. Add to the artificial dietary quite minute amounts of material extracted, *secundum artem*, from animal or vegetable tissues and it supports growth quite normally.

It appears as though we shall have to extend our concepts concerning efficiency in rations beyond the range of nutritive values in the stricter sense and speak of the indispensable "physiological actions" of certain constituents. Part of dietetics is to become part of pharmacology!

I have avoided going into details with reference to this matter, as others will follow me who are qualified to speak concerning them. I have said enough to suggest that something like a revolution is about to upset much of our dogmatic teaching concerning animal nutrition. It is well, I think, that

the public should know how much there is yet to be done by way of observation and experiment before our knowledge of this important subject can be said to be in any way complete. How far the newer conceptions that I have touched upon will intrude into practice the future alone can tell. The united efforts of the practical stock-breeder and of the laboratory investigator will be required before the degree of their importance can be determined.

ACTIVE CONSTITUENTS OF GRAIN

(PROF. LEONARD HILL)

In order to test the worth of the claims made during a recent newspaper agitation as to the superiority of standard bread, we obtained a large number of young rats and mice, caged them in lots of twenty in the same way and fed some lots on white, some on standard and some on whole meal flour and water. We soon found that white flour was not a food on which life could be maintained, whilst standard or whole meal flour proved to be very much better. White flour to which we added the germ sufficed to maintain the animals in health and in some cases through two or even three generations. The fashion for standard bread has died away because people prefer the colour and taste of white bread. White bread is a better foil to other tastes and so adds to the pleasures of the palate.

White flour also bakes into a loaf of better quality. It is a matter of indifference to most of us whether we eat white bread and discard the active subtle principles in the outer layers of the wheat berry, because we obtain these principles from meat, milk, eggs, the growing tips of vegetables, etc. In the case of slum children or the children of the Labrador fisher-folk, fed on white bread and tea, however, it is a matter of great moment; such a diet is the cause of beri-beri (rampant in Labrador) and probably of scurvy and contributes to other slum diseases.

Flack and I have succeeded, by adding an extract of bran and sharps to the dough, in making a white loaf excellent in taste and flavour and containing the principles necessary for life. On this bread we have successfully fed pigeons, whilst the birds in the control experiment fed on the best ordinary white bread all died. There is no reason therefore why a white bread should not be made containing the essential active substances.

In speaking of these observations I wish to acknowledge the priority of Dr. Gowland Hopkins, whose usual modesty had prevented him from putting forward his own important contributions to the subject. Ill health delayed Dr. Hopkins from publishing work which showed the deficiency of white flour as a life-sustaining food. I leave it to Dr. Casimir Funk to discuss the chemical nature of these active substances which form so small and so essential a part of foodstuffs. As these substances are destroyed by heating to 170° F. and are removed by modern milling processes, it is obvious that great danger lies in diets restricted to tinned food and white bread. Our supplies of fresh natural foods must be maintained. Frightful suffering and loss of life have been caused by the polishing of rice, a milling process introduced merely to make the rice white and please the eye of the buyer. This rice indeed has proved a whited sepulchre and it has taken years of work to trace home the causation of beri-beri to it.

AN EXPLANATION OF BERI-BERI

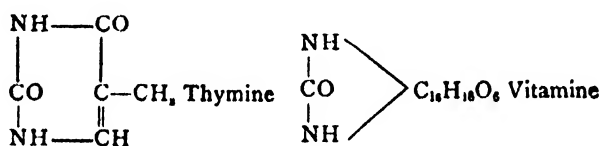
(DR. CASIMIR FUNK)

A substance has been isolated recently in what appears to be a pure condition from rice-polishings, which it is suggested should be named vitamine. It crystallises in colourless needles, which melt at 233° ; the results of the single analysis, which the amount of material at my disposal permitted, indicated the formula $C_{17}H_{20}N_4O_7$. The administration of this substance (about 0.02 gm.) to pigeons suffering from polyneuritis (beri-beri) effected a rapid cure. The small proportion obtained, however, did not allow of many such curing experiments being performed and as the substance was not recrystallised doubts of its purity might be entertained. A confirmation of these facts was therefore absolutely necessary. In the first instance yeast, which is known to be curative, was chosen as the source of the material, as it was likely to give a better yield than rice-polishings. It was of great interest to see whether yeast contained the same substance as rice-polishings or only an analogous compound. It was found possible to prepare a substance apparently identical with that present in rice-polishings. The substance occurs in the fraction containing the pyrimidine bases, which are, in fact, more or less precipitated by the agents used in separating it.

Its curative effect was amply demonstrated by experiments on pigeons, a dose of 2.4 cgm. being necessary.

The aqueous solution is neutral and not acted upon by acids. On boiling with copper oxide no copper salt is formed and therefore it is not an amino-acid. When recrystallised from dilute alcohol the substance melts at 233° , which is the same as that at which the curative substance from rice melts. As the substances behave alike they must be considered to be identical. It is precipitated in a pure state by mercuric acetate as well as by silver nitrate but not by mercuric sulphate nor by the nitrate.

All these properties suggest that the curative substance is a pyrimidine base analogous to uracil and thymine and that it is probably a constituent of nucleic acid. On this view the two nitrogens would be combined as in other pyrimidine bases in the form of an ureide:



Only a constitution of this kind would explain the neutral character of the substance and its analogy with other pyrimidine bases.

The curative substance was also isolated by analogous methods from milk (this fact being very important in connexion with infantile scurvy) and bran. Everything suggests that in all these cases the curative substance is identically the same. Further, a substance curing avian polyneuritis was found in lime-juice, which is at present being more closely investigated. These experiments throw an entirely new light on the physiological importance of the nucleic substances.

MORE DIFFICULTIES FROM THE PRACTICAL SIDE

(DR. DAVID WILSON)

The values obtained by the methods in vogue are not a sufficient indication of the relative feeding quality of home-grown foods—grass, roots and fodders—which form the greater part of farm rations. For example, analyses made of samples of grass from five pastures gave the following indecisive results¹:

¹ *Trans. Highland and Agric. Society*, 1894, pp. 411-16.

Reference numbers	3	2	1	5	4
	Poorish but productive pastures.			Fattening pastures.	
Annual value of pastures per acre	16s.	20s.	26s.	60s.	70s.
	In 100 parts dry matter of grass.				
Protein	12.25	11.37	11.37	11.60	12.25
Amides, etc. . . .	4.81	3.07	5.36	1.00	1.06
Ether extract	2.20	2.10	3.75	3.95	4.57
Carbohydrates	50.69	52.31	46.47	52.35	51.62
Woody fibre	19.85	18.30	21.50	19.90	20.10
Ash	10.20	12.85	11.55	11.20	10.40
	100.00	100.00	100.00	100.00	100.00

Bullocks fed only on turnips and straw grown in certain districts increase in weight as rapidly as they do in other districts where they receive 4 lb. of good cake daily in addition. Three sets of turnips obtained from different sources were analysed by Aitken¹; each set consisted of two sacks distinguished by numbers only, the one containing good fattening turnips, the other roots of very poor quality. In every case he selected the poor turnips as those likely to be best for feeding. If analyses gave no information, the odds would be 7 to 1 against his making the wrong choice three times running. Lawes,² Warrington,³ Hendrick⁴ and Hall and Russell⁵ may be quoted as confirming this inadequacy of present methods on which scientific values are now based; as such methods are inadequate to measure the feeding quality of the main part of the ration, they cannot show the kind and quantity of cakes or grains required to supplement an unknown deficiency.

Ingle⁶ has tabulated and discussed British feeding experiments, dealing with 989 cattle and 2,765 sheep. His graphs compare separately "Digestible Protein," "Digestible Starch," "Total Digestible Matter" and "Albuminoid Ratio" with increase. If "Starch Equivalent" and "Digestible Protein," as ordinarily calculated, are actually a measure of feeding power, such a large number of animals, viewed statistically, should

¹ *Trans. Highland and Agric. Society*, 1889, p. 253, and 1893, p. 356 (foot).

² *Agric. Student's Gazette* 1892, p. 1.

³ *Ibid.* 1893, p. 6.

⁴ *Trans. Highland and Agric. Soc.* 1911, p. 191.

⁵ *Agricultural Science*, iv, pp. 366-70.

⁶ *Trans. Highland and Agric. Soc.* 1909, pp. 196-254, and 1910, pp. 168-257.

show some definite relation between these units and increase. Some of Ingle's graphs do show a certain measure of correlation but viewing the whole display in light of the law of error the "Starch Equivalent" and "Digestible Protein" are correlated with such widely different feeding effects that they must stand for different things in the different rations.

Further, the conclusions drawn from such correlation as exists in these graphs do not confirm Kellner's standard rations. They indicate a lower protein requirement and show no correlation whatever between "Albuminoid Ratio" and increase in cattle or sheep.

The heterogeneous nature of all the analytical units on which scientific feeding values are based seems a sufficient reason for these failures.

An ordinary analysis gives :

(1) "Protein" or "Albuminoids." Every animal ration, to be efficient, must contain a certain minimum of certain proteins. But a minimum of specific proteins is a very different thing from a minimum of insoluble and precipitated nitrogen multiplied by six and a quarter. Even in a mixed ration, an animal may have to consume a great superfluity of other proteins before it obtains the necessary amount of specific proteins. The various amounts of protein recommended by different authorities and the great divergences in Ingle's charts indicate that this difference in effect of a unit of mixed proteins frequently occurs in practice.

(2) "Amides, etc.," account for a large proportion of the nitrogen in home-grown foods. Theoretically different quantities and kinds of concentrated food would have to be added to turnips and straw according to the method adopted in evaluating this group of varying composition and unknown function.

(3) "Ether Extract" is also a varying mixture. In young grass only about 35 per cent. is fat¹ and all oil is not linseed oil.

(4) "Carbohydrates" and "Fibre" are equally heterogeneous. The "Fibre" of undecorticated cotton cake and of swedes and the "Carbohydrates" of maize and wheat straw are, so far as our analyses go, the same things. We are therefore entirely dependent on average "digestibility" and "value" factors and in the case of the foods most largely used here the "probable error"

¹ *Highland and Agric. Society Trans.* 1889, p. 44.

of these factors is great. The digestible protein in swedes is given by Kellner as '3 per cent.¹ on the sole authority of an experiment upon two sheep, one of which increased in weight seven times as much as the other.²

The digestibility of the fibre in turnips varied from 0 to 100 per cent.,³ and that of protein in oat straw from 12 per cent. to 50 per cent.⁴ The result of a "digestibility" or "value" experiment is true only of the particular food, in the case of a certain animal, under certain restricted⁵ conditions and cannot be usefully generalised by applying it to the mixed units of ordinary analysis.

A farmer learns from experience how he must supplement his own roots and fodders. Moreover the good cattleman studies the individual animals and keeps their appetite fresh. "It is the master's eye that fattens the cattle." His rations will fall within certain limits and generally our present science is not warranted in making these limits closer. The primary object of research on feeding values in this country is not to inform practical feeders how to construct their rations but to increase the feeding quality of the foods we grow, which form the main part of these rations. We know what an efficient measure of the required quality did for the sugar-beet industry. If we had an equally true measure of the feeding quality of home-grown foods there is reason for hoping it would in some similar degree benefit the agricultural industry. We could select, breed, manure and cultivate with confidence and the tools which got us increase of feeding quality would help us best to use it.

CERTAIN OIL FOODS

(PROF. HENDRICK)

In most of the previous experiments on the substitution of other fats for butter fat, cod liver oil has been used and the opinion is consequently prevalent that this is the only oil which can properly be used. The general purpose of the experiments now described, in which calves were fed with

¹ *Scientific Feeding of Animals* (Goodwin's Trans. 1909), p. 370.

² *Bled. Centr.* 20, pp. 12-19, and *Chem. Soc. Abstracts*, 1891, p. 595.

³ *Scientific Feeding of Animals*, p. 385.

⁴ *Ibid.* p. 383.

⁵ *Highland and Agric. Soc. Trans.* 1893, p. 344.

cotton seed oil as a substitute for butter fat, was to demonstrate the practical economy of using separated milk and oil in place of whole milk in feeding ordinary commercial calves. Cotton seed oil was chosen as a comparatively cheap and easily obtained vegetable oil which is extensively used in human food and is known to be wholesome. Another reason why it was chosen was that certain practical men, even of the intelligent and educated class, were profoundly sceptical as to its value as a food for calves. Their suspicion appeared to be based on the general unsuitability of cotton cake as a food for young stock.

Three series of calves were fed during the experiments. Each series consisted of three lots fed as follows:

Lot I. Whole milk till time of weaning.

Lot II. Whole milk till three to five weeks old, after which either separated milk and cod liver oil or separated milk, cod liver oil and a meal gruel were gradually substituted for whole milk.

Lot III. Whole milk till three to five weeks old, then the place of whole milk was gradually taken by separated milk, cotton seed oil and a meal gruel.

After weaning, the calves were all treated similarly till about two years old, when they were sent to the butcher fat. Records of the weights were kept till the time of slaughter, when the carcase weights and a report on the carcasses by the butcher were obtained.

The following table gives a summary of the results:

	Lot I. Whole milk.	Lot II. Cod liver oil.	Lot III. Cotton seed oil.
Total number of calves	14	15	15
Average weight at start	109 lb.	113 lb.	107 lb.
" " weaning	309 "	290 "	280 "
" increase when weaned	200 "	177 "	173 "
Average cost of feeding to time of weaning (per calf)	£3 19s. 3d.	£1 7s.	£1 5s. 9d.
Average cost of food per pound of increase .	4'82d.	1'83d.	1'79d.
" weight when sent to butcher .	1,150'3 lb.	1,117 lb.	1,078'3 lb.
" increase " " " .	1,041'3 "	1,004 "	971'3 "

The table shows that there is little difference, on the average, between the increases made by calves fed with cotton seed oil and those fed with cod liver oil. The cost of the cotton seed oil feeding was slightly less. There did seem to be a distinct difference in favour of the whole milk calves till the

time of weaning; after that there was no significant difference and at the time of slaughter the differences between the lots was so small as to be within the limits of experimental error. So far as the evidence of these experiments goes, it shows that cotton seed oil is as suitable as cod liver oil as a substitute for butter fat in feeding calves.

I have long recognised that mere chemical analysis and energy value or starch value do not tell all that is required in order to enable us to determine the position and value of a feeding stuff. At one time and that not so long ago energy values, albuminoid ratios and chemical analyses were looked upon as almost the whole gospel of the nutrition of farm animals; this period of development is still the one represented in the text-books of agriculture and agricultural chemistry. Now it seems that the pendulum is swinging strongly over to the other side and it is desirable to utter just one note of warning. In the reaction against the overgrown claims of an old school, do not let us go to the other extreme and lose hold of what was true and right in their work. Although our methods of food analysis are very imperfect and all our work is vitiated by this and by the great individual variations which occur in experiments with animals, still if there be one solid basis of well-established fact which we hold on to as scientifically sound and unassailable it is the energy values of food-stuffs and nutrients. Moreover we are on sure ground in maintaining that energy values for the animal are the same as for the inanimate machine, making due allowance for the products of combustion obtained in each case.

THE MAGNITUDE OF THE ERROR IN NUTRITION EXPERIMENTS

(PROF. R. A. BERRY)

It is desirable to direct attention to the magnitude of the experimental error in nutrition experiments on animals, dealing especially with the case of pigs.

In an experiment carried out at the experiment station of the West of Scotland Agricultural College in 1911, in which seventy-six large white pigs were used with an average initial live weight of 77·6 lb. equally divided as to sex and all fed on the same ration during fourteen weeks, the probable error for

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one animal was 12·1 per cent. of the live-weight increase. Calculating from the results of previous experiments extending over the years 1905-8 and choosing only those lots which were fed on the same or similar diets, numbering 102 pigs having an average initial live weight of 97 lb., the probable error for one pig works out to 13·7 per cent. of the live-weight increase. Both sets of figures give practically normal frequency curves. The differences mean that twenty-one or twenty-seven pigs are necessary to determine with any degree of certainty a difference of 10 per cent. between different foods and that thirty or thirty-eight pigs are required to determine a difference of 10 per cent. in either direction. These differences, though not large, point to the advisability, when calculating the probable error, of taking into account age and weight of animal at commencement of the experiment and of considering whether the data are drawn from one complete experiment or from several experiments extending over a number of years.

Fifty female pigs in the latter experiment gave a probable error of 13·5 per cent. of the live-weight increase and fifty male pigs 13·8 per cent.

Wood gives about 14 per cent. of the live-weight increase on the probable error for cattle and sheep. His method of calculation is followed here.

In connexion with the variation and sampling of oat straw, using data from a hundred individual straw analyses, the probable error was very great and varied according to whether it was calculated on the percentage of nitrogen, the total weight of nitrogen or the dry matter of individual straws, respectively. Except in the case of the total weight of nitrogen the frequency curves were abnormal. Similar variations were found in the probable error and frequency curves calculated on the different constituents of the mangel.

A NOTE OF CAUTION

(DR. CROWTHER)

From no part of his work has the agricultural chemist in the past derived less real satisfaction than from his efforts to harmonise farm practice in feeding animals with the views dominant from time to time amongst physiologists as fundamental principles of animal nutrition.

His difficulties arise largely from the patent incompleteness of physiological theory on the one hand, on the other from the manifold imperfections of the methods commonly used in determining the content of utilisable nutrient matters in foods.

During many years past it has been the common practice to compare the merits of different foods or rations in terms of their content of protein, fat, carbohydrates and "fibre," without taking into account any quantitative differences in the make-up of the materials comprised under these designations. It has further been the custom to insist in the case of each class of stock upon a definite "balance" being maintained between the protein and non-protein constituents of the ration ("albuminoid ratio") as a matter of fundamental importance. Fats, carbohydrates and any excess of protein beyond the indispensable minimum have been regarded as mutually interchangeable in the proportions of their "isodynamic equivalents." The application of these views to farm practice, however, has met with overwhelming difficulties from the start. There are difficulties which any system will meet with necessarily, such as the great variability in the composition of the foods that form the staple of the ration and the individual variations in feeding capacity between different animals. But even in cases in which these general difficulties have been largely overcome, there has often been a hopeless discordance between theory and practice. Rations esteemed, from theoretical considerations, to be of equal value, have frequently given widely different results in practice. Albuminoid ratios condemned outright by theory have in innumerable cases proved in practice to be in no whit inferior to the optimum ratios of theory.

In the main, doubtless, the method has served the useful purpose of correcting gross errors in feeding but its application is so uncertain that it has never won the confidence of the skilled feeder and voices have not been wanting to suggest that theory has as yet little or nothing to offer in the way of guidance to experienced practice.

Some explanation of the discrepancies has been suggested by the results of recent research on nutrition. We realise now clearly that all proteins are not to be treated as mutually equivalent and that "amides" need often to be taken seriously into account. We know further that the attainment of the full nutritive value of certain foods is conditioned by the presence

in them of small quantities of an ingredient or ingredients whose character has not yet been determined. Further we have reason to believe that the interchange of fat and carbohydrate is safe only so long as certain minimum amounts of each are present in the ration. Lastly we may mention the factor of palatability, which has been found to exercise an influence, within certain limits, upon the nutritive efficiency of foods consumed by farm stock.

Further blame for the discrepancies alluded to above might easily be put upon the crudity of the analytical units in terms of which the composition of foods is expressed.

Protein, carbohydrate and fibre, as commonly returned in the analysis of foods, are not definite chemical individuals but more or less complex groups of ingredients; the amounts of these present are arrived at, moreover, by methods which are not of a high order of accuracy. In the case of "carbohydrates" indeed, for want of a feasible method, no attempt at a direct determination is made but the amount is simply arrived at by difference. Added to these shortcomings are the further crudities of the estimation of digestibility. Of these only one need be mentioned—the assumption that all material removed from the food during its passage through the animal has been "usefully" digested.

In the face of all these complications and difficulties, it is obviously impossible to devise any system of computing food values that will give more than a rough estimate.

For the purposes of the farm, however, the rough estimate will, in most cases, be sufficient and it would obviously be foolish to abandon even the old method of arriving at such an estimate, without further test of its value when modified in accordance with the outcome of recent research.

It will be generally agreed that, *provided it be satisfactory in other respects*, the nutritive value of a ration will be determined by the amount it contains of assimilable protein, fat and carbohydrate. Assuming that the ration is suited in bulk and character to the animal and consists of sound foodstuffs, the chief "other respects" that need to be satisfied will be, so far as present knowledge informs us, the character of the proteins and "amides" present and the inclusion of the little-known ingredients whose presence, though only in minute amount, is essential for the efficient utilisation of the food in the body.

In a simple ration, such as is often fed to pigs, there is risk

that these last-named requirements may not be adequately satisfied but it is probably only rarely, if ever, that such a difficulty will arise with the more complex rations of roots, fodder and concentrated foods commonly given to the other classes of farm stock. With a basis of roots, hay or grass and straw—given fair quality—no difficulty is ever experienced in devising a ration on which “thrifty” animals will maintain a good rate of growth, so that apparently these materials, as a rule, effectively supplement any deficiencies of constitution in other foods with which they are blended. We are probably committing no serious error, therefore, in assuming that the nutritive value of such rations is determined essentially by their content of digestible protein, fat and carbohydrate and it remains to devise a satisfactory method of evaluating this content for practical purposes.

As yet only one method has been put forward which can be said to rest upon a substantial basis of experimental investigation, viz. the method developed by Kellner, which for convenience may be referred to as the “starch equivalent” method.¹ This method is based upon the classical measurements by Kellner of the value to the fattening adult ox of pure preparations of protein, oil and carbohydrate and also of a variety of common feeding stuffs—in all, upwards of seventy experiments. In these experiments the results of the feeding were gauged by careful determinations of the gain of carbon and nitrogen by the body and in every case the material under investigation was compared directly with starch. In this way the relative values (starch = 1) to the fattening ox of the different nutrients when fed separately in pure, easily digested state were found to be :

Digestible starch	= 1'00
„ fibre (cellulose)	= 1'00
„ protein	= 0'94
„ oil	= 2'41

In applying these values to the computation of the starch-equivalents of ordinary foodstuffs, it is necessary to make allowance for factors that tend to reduce the nutritive value of the foodstuff, such as the labour of mastication, etc. In other words, the “availability” (Wertigkeit) for productive purposes of the digested matter must be taken into account.

¹ Kellner, *Die Ernährung der landwirtschaftlichen Nutztiere*, iv. Aufl. 393 ; Goodwin, *Journal of the Board of Agriculture*, xviii. 721.

According to Kellner's measurements, this is rarely less than 95 per cent. in the case of easily digested foodstuffs but may be as low as 30 per cent. in the case of tough, fibrous material, such as wheatstraw. Such "percentage availabilities" of a large range of feeding stuffs have been tabulated by Kellner. In the case of the more fibrous foods, however, he prefers to base his correction of the theoretical starch equivalent upon the proportion of crude fibre in the food, since it is this proportion that largely determines the labour required for mastication and digestion of the food.

A further difficulty in the computation of starch-equivalents arises from the uncertainty as to the value which should be attached to the non-protein nitrogenous ingredients of foods. Kellner treated them as valueless for productive purposes but this procedure perhaps hardly does full justice to these "amides."

It remains to be seen how this method will stand the test of application in practice. Its validity can only be thoroughly tested by the records of experiments, conducted upon a relatively large scale, in which the exact consumption of digestible protein, fat, carbohydrate and fibre is recorded. Such experiments have, as yet, been carried out but rarely in this country. A very large number of carefully conducted feeding trials have been carried out but in hardly a single case has any determination of digestibility been made and in the great majority of cases information is lacking with regard to the composition of the roots, hay, straw or other home-grown foods consumed by the animals. Without this information, however, it is impossible to make any stringent test of the validity of the starch-equivalent as a measure of nutritive value. All we can do is to make a rough test by assuming for the home-grown foodstuffs—of all foodstuffs the most variable in composition—an average composition and digestibility, together with similar assumptions with regard to the digestibility of any other foods of known composition included in the ration.

It is not to be wondered at that the starch-equivalent method has not survived with complete success every such rough test that has been applied to it. Little weight can be attached, however, to the results of such imperfect tests based upon the results of one or two feeding trials. Of greater interest is the comparison of the relative productive values of foodstuffs as shown by their starch equivalents, with the

average results obtained in feeding trials upon a large scale or frequently repeated, such as those conducted in Denmark by Fjörd and Friis and in Sweden by Hansson.

The comparison with the Danish and Swedish results is the more interesting in that the latter have reference to the relative values of the foods in milk-production whereas Kellner's experiments, upon which the method of computing the starch equivalents is based, were measurements of fattening increase. Below are given the equivalent quantities of a variety of foods of different types, as deduced from their average starch equivalents, alongside the corresponding data given in three separate tables which are based solely upon practical feeding trials. In each case, wheat is taken as the basis of comparison.

EQUIVALENT QUANTITIES OF FOOD

	From starch equivalents (Kellner's averages). ¹	Danish scale. ²	Müller & Wendt's scale ³ (based upon Swedish trials).	Lawes & Gilbert's scale. ⁴
Wheat	1	1	1	1
Bran	1'5	1	1'1	1 25
Oil cake and similar foods	'9-1'1	1	85-1	'9-1'1
Clover hay . . .	2'2	2	2'5	2
Meadow hay . .	2'3	2'5	2'6	2'1
Mangels	11	10	10	13
Turnips	15	12	12'5	19
Straw	4'2	4	4	2'5
Green fodder . .	7-9	10	7'5-11	—
Potatoes	3 8	4	5	8'5

With one exception (bran), the degree of concordance shown in this comparison between the "theoretical" (starch equivalent) feeding values and the "practical" feeding values is little less than remarkable and it must be obvious, even to the layman, that a method which, even when applied in somewhat rough fashion, can give such an approximation to the results of practice, is worthy of a thorough and extended trial. There can be little doubt that when its foundations have been more thoroughly explored and the limits of its applicability more precisely defined, it will become a permanent instrument in controlling feeding practice on the farm.

¹ *Loc cit.* pp. 582-93.

² *Journal of the Board of Agriculture*, April 1905, 23.

³ *Grundsätze einer wirtschaftlichen Ernährung der Milchkühe*, Berlin, 1909.

⁴ *Journal of the Royal Agricultural Society*, 3rd Series, viii. 698 (1897).

THE SPECTRE OF VITALISM

By HUGH S. ELLIOT

1. Article by Dr. J. S. Haldane on "The Relation of Mind and Body," in *SCIENCE PROGRESS*, October 1912.
2. Presidential Address by Dr. J. S. Haldane to the Physiology Section of the British Association, 1908.
3. Becquerel Memorial Lecture to the Chemical Society, 1912. By Sir Oliver Lodge.
4. Article by Sir Oliver Lodge on "Life and Professor Schafer" in the *Contemporary Review* for October 1912.
5. Article by Sir Oliver Lodge on "Uncommon Sense as a Substitute for Investigation," in *Bedrock* for October 1912.
6. *Science and Philosophy of the Organism*. By Hans Driesch. (London: A. & C. Black, 1908.)
7. *Involution*. By Lord Ernest Hamilton. (London: Mills & Boon, 1912.)
8. *On the Inheritance of Acquired Characters*. By Eugenio Rignano. (Chicago: Open Court Publishing Co., 1911.)
9. *Is the Mind a Coherer?* By L. G. Sarjant. (London: George Allen & Co., 1912.)

MEN may be roughly classified into the two divisions of those who believe in ghosts and those who do not. In old days *everybody* believed in ghosts: everybody had his own private ghost, which he gave up when he died: there were besides a number of ghosts specially connected with departed personages; and in addition to these, there was an army of ghosts on the loose, so to speak, not specially connected with any human individual. In short the ghost population vastly exceeded the population of material human beings.

In modern times the population of ghosts has undergone a very serious decline. A great many people do not believe in them at all: and those who do no longer credit them with the powers that their ancestors were supposed to possess. This degeneration among ghosts has clearly been brought about by the development of science: for the more we learn how things happen, the more conscious do we become that ghosts do not play the part in the causation of events that they were supposed to play: indeed it is now somewhat widely believed that ghosts play no part at all and that all events have material,

not spiritual, causes. Yet it is well to remember that the primitive impulse of mankind is to believe in ghosts: that, in the absence of scientific explanations, spiritual "explanations" are commonly put forward and that even in the presence of scientific explanations, a ghost is a shifty sort of character, not easily driven finally out. The "will to believe" is so strong that, even now, those who believe in ghosts of some sort or other greatly exceed in number those who do not.

Among physiologists, those who believe in ghosts are called vitalists and those who do not believe in ghosts are called mechanists. The latter, who among physiologists are greatly in the majority, affirm that all animal activities are due to physical, chemical and mechanical forces acting in accordance with laws the same as those which hold for the inorganic world. The vitalists on the contrary declare that material forces alone cannot account for all the manifestations of life, though they do account for most of them: they say, however, there is a residue of vital manifestations not so accountable and they throw what they deem to be a flood of light over the whole situation, in affirming that these vital manifestations are caused by a vital force. The uneducated man apparently finds comfort in the explanation. His propensity towards believing in ghosts naturally disposes him to acquiesce in the presence of such forces as ghosts might be expected to exert. It is now many years ago since du Bois Reymond attempted to exorcise what he aptly called the "spectre of vitalism": but that spectre in an attenuated form still continues to haunt a few who have predispositions towards it. I have already elsewhere attempted to refute the general doctrine of vitalism.¹ My task here is to review a certain number of recent publications which have fallen from the pens of modern believers in ghosts.

THE VIEWS OF DR. HALDANE

And first let me deal with the views of Dr. Haldane, as stated in the October number of this review. There is indeed nothing in it which can very easily be replied to: for Dr. Haldane does not argue but confines himself to setting forth a series of rather odd opinions, without furnishing the clue as to how he came by them. The mechanistic theory therefore is in no way

¹ *Bedrock*, October 1912.

injured by his paper: except in so far as the authority of Dr. Haldane's name may injure it. I venture however to criticise a few of his utterances. "We cannot," he says, "express the observed facts by means of physical and chemical conceptions but must and do have recourse to the conception of organic unity." What is that conception? Here is an attempt to shift the required explanation from the ground of science to metaphysics: which is virtually to abandon the problem altogether. Then there follows the stock argument: "Living organisms are distinguished from everything else that we at present know by the fact that they maintain and reproduce themselves with their characteristic structure and activities." Even this argument is disputed by Prof. Schäfer and others; but let us assume it to be the case: what follows? Has not every substance its own peculiar properties which differentiate it from other substances? The fundamental constituents of protoplasm are bodies of immense molecular complexity: it is to be expected therefore that such substances will display properties different from those of inorganic substances. The phenomena of growth occur, in crystals, in the most elementary chemical substances. It may be true that there is small analogy between crystal and organic growth. But there is also small analogy between the substances considered. If such a simple substance as sodium chloride possess the property of growth under certain conditions, we need not be surprised that protoplasmic substances possess a corresponding property in immensely greater variety and complexity. The question at issue is *not* whether growth and reproduction occur in the inorganic realm: it is whether the complexity and variety of these phenomena in the organic realm are such as to be totally out of proportion to the molecular complexity and variety of the substances built up in protoplasm. But these substances are still unknown: and he would be a bold chemist who would assert that their united formulæ are too simple in character to serve as foundation for the functional manifestations of protoplasm. I confess I have always been puzzled as to why any one should attach the slightest importance to the argument that living organisms have peculiarities different from those of inorganic matter. That the substances in protoplasm have properties not found in other substances surely bears in no respect upon the question of vitalism and mechanism.

The next quotation from Dr. Haldane on which comment may be made is this: "In the argument that all the conscious behaviour of a man or animal is ultimately dependent on physical and chemical stimuli from the environment, acting on the physical and chemical structure of the body, the whole question is begged from the outset; for the assumed physical stimuli and physical structure do not behave as such." I do not understand how the term "behaviour" can be applied to stimuli or structures: still less, as here alleged, how either a stimulus or a structure can behave as though it were neither a stimulus nor a structure but as something else—a ghost, no doubt. But in any case, we may ask for further information as to how the question is begged in arguing that conscious behaviour is dependent on physical stimuli. That proposition may be either true or untrue; but it is perfectly clear and straightforward itself and begs nothing. Dr. Haldane's whole attitude is metaphysical. Metaphysical questions may possibly here be begged: I do not know whether they are and I certainly do not care. Science will not stop, merely because ghostly questions are begged! "The great mistake of mechanism," says Dr. Haldane, "is to lose sight of the wider point of view which shows us that in physical or indeed any scientific investigation we are always dealing with partial aspects of reality." Since this mistake is common to mechanism as well as every other scientific theory, I suppose that mechanism, if proved, would have the same sort of validity as other scientific truths: and that is all we want. But the general type of the argument is unsatisfactory: it is meeting a scientific theory with a metaphysical refutation. If the scientific theory be untrue, it is surely susceptible of a scientific refutation or criticism, at all events. And so we go on: "Conscious personality is the truth of the body and its environment." I have no idea what this sentence means: nor how the body can have a truth. But things get worse and worse: "Just as biological facts have taught us that the life of each individual cell or organism is only part of a wider life, so have ethical and religious facts shown that the individual personality in its full realisation is the expression of divine personality, which alone can be the ultimate truth of all existence." If this is intended as hostile to mechanism, it surely is the weakest of arguments. That the divine personality is the truth of existence is the sort of thing

one finds in books on theology. Happily books on theology are fast giving way to books on science. The sentence is meaningless: and even if it were not, its logical basis of "religious facts" is utterly flimsy. Let us not import into the vitalistic controversy arguments founded in the rapidly passing superstitions that are proper only to the childhood of civilisation.

Dr. Haldane has given a more complete account of his views in his presidential address to the Physiological Section of the British Association in 1908. But here again, a critic can find little to lay hold of: so elusive are Dr. Haldane's methods. He uses in the main two arguments: (1) the argument from teleology; (2) the argument from the inadequacy of physico-chemical explanations. With the teleological argument I have dealt elsewhere¹ and need only briefly recapitulate what I then pointed out. Dr. Haldane puts the matter in some such form as this: Physico-chemical laws act blindly in their operation; "purpose" is foreign to them and they are inadequate to express purposefulness in events. But physiology shows a "teleological ordering" of matter and energy. Every function is nicely adapted to the needs of the organism and thus possesses a purposefulness not to be explained by mechanical laws. The argument fails because the premisses are erroneous: teleological events are *not* incompatible with mechanism; a truth that ought to be patent since the discovery of Natural Selection. For here we have a teleological event—namely, evolution towards increasing complexity of structure and specialisation of function—following as a result of laws wholly mechanical: namely, the extinction of the organisms least fitted to survive. It is true that some biologists consider "natural selection" inadequate to account for evolution. They have suggested other factors; but all these suggested factors are of a mechanical nature. In short, there is almost universal agreement that evolution is produced by mechanical causes. We must, then, admit either that evolution is a blind, purposeless process; or, if it have a purpose, that that purpose is expressible in mechanical terms. In other words, there are not two kinds of events—the purposeful and the unpurposeful. The "purposiveness" of an event arises solely from our point of view; it is not an attribute of the object but of the subject. *Any* natural event may be

¹ *Bedrock*, October 1912.

regarded as purposive if we orientate our minds in a certain way towards it; but *all* natural events are none the less mechanical, physical, chemical, etc., in their causes and mode of working. So, in referring to the teleological harmony of the bodily functions, Dr. Haldane is only naming one of the results which most biologists attribute to the blind operation of natural selection. It is a striking example only because of the extreme perfection of the adaptation between the organs of the body. Not the extremest vitalist would deny that natural selection, a mechanical factor, may bring about adaptation: that the fauna of a country is adapted to the climate for the simple reason that non-adapted varieties could not live. And if adaptation of a simple kind be thus mechanically explicable, if adaptation itself be a mere mechanical event, then vitalists cannot look to it for arguments against mechanism.

Dr. Haldane's second argument is a very common one. "The conceptions of physics and chemistry are insufficient to enable us to understand physiological phenomena": hence we must pass to some vitalistic theory. That argument has lain at the base of every myth since the world began. Here is a strange event: we do not see how natural forces could have compassed it: therefore ghosts did it. It is the primitive tendency to attribute animism to whatever we cannot understand. I shall comment later upon this argument in connexion with the work of Driesch. With reference to Dr. Haldane, I need only refer further to his statement that biology "deals with a deeper aspect of reality" than physics and protest once more against the introduction of metaphysical conceptions into science. "Reality" is not a stratified deposit into which we may penetrate more or less deeply: all scientific truths are equally real for the man of science, those of physics not less so than those of biology.

THE VIEWS OF HANS DRIESCH

One of the most frequently quoted of all authorities in favour of vitalism is Hans Driesch, whose *Science and Philosophy of the Organism* is a deliberate attempt to re-establish that discredited doctrine on a secure foundation. It will be necessary, therefore, to devote some space to the examination of his three proofs of vitalism.

The first proof is introduced by a long account of the facts of morphogenesis or the development of the individual organism. Driesch commences with the fact that, if we go back early enough in the history of an embryo, we reach a time when all its parts are equally capable of giving rise to an adult organism: when, in fact, there has been no differentiation of parts and each portion of the embryo is as capable as any other portion of developing into any of the specialised structures of an adult organism. Any portion of an embryo which answers to this definition is called by Driesch an equipotential system. If each embryonic part be equally capable of developing into *any* of the varied adult parts, what factors are they which control the development into a harmonious whole? Driesch shows, in the first place, that the absolute size of the system and the relative position of any point in it are factors in accounting for the trend of development. But he points out that they cannot alone explain development: there remains the "prospective potency" of the system—that is to say, its power of developing in certain directions which terminate in the structure of an adult organism. What is this potency? Let us refer to it as E. Driesch then considers every possibility that can be named as to the nature of E. From the mechanistic point of view there are, he says, three possibilities. There is, firstly, the possibility that "formative stimuli" are sufficient to account for development; there is, secondly, the possibility of a chemical basis; and there is, thirdly, the possibility "of a real machine in the system," one more or less resembling that suggested by Weismann. Driesch takes two pages to refute the first possibility, four pages to refute the second possibility and four pages to refute the third; and the very next page is headed by the legend, "Vitalism Proved."¹ This startling announcement is founded on the following logical process: There are four possibilities as to the nature of E; it may be either any one of the three already named or it may be "a true element of nature." But it has been proved that it is none of the three first named; *therefore* it must be the fourth. Henceforward it figures under the title of "entelechy."

I am aware that, in pausing to point out the sundry fallacies involved in the above argument, I shall be casting reflections on my reader's perspicacity. Nevertheless, since Driesch ap-

¹ The actual legend is "The autonomy of morphogenesis proved": but Driesch defines "autonomy of morphogenesis" as synonymous with "vitalism."

parently never has been answered and since many people interpret silence as inability to reply, it is desirable to make a few obvious criticisms.

The method is that which is sometimes called *per exclusionem*. The first stage is to prove that there are only a limited number of possible explanations of some phenomenon. The second stage is to prove in turn that all these explanations except one are false. It then follows that that one must be true. Fallacies may enter at any step in the argument; but they are most likely to occur in the first stage. It must always be an exceedingly difficult matter to prove that the range of possible explanations is limited to four or five or any other number. It is difficult to imagine a process by which one could make sure that no alternative possibility had been overlooked: that all conceivable theories have been marshalled in the field and that the suggestion of any other theory at any future time in the history of science is inconceivable. Yet, unless that be done, the whole method lapses. The second stage presents a further opportunity for the introduction of fallacies. Each suggested explanation that is refuted furnishes a loophole for error in the refutation; and a single error at any part of the argument vitiates the whole. It is obvious, therefore, how untrustworthy and difficult the method *per exclusionem* must always be. Has Driesch recognised that untrustworthiness? I have already observed that he takes ten pages to dismiss the three possible explanations which he suggests; that is, for the second stage. *But he has clean forgotten all about the first stage.* The reader is left in bewilderment to work out for himself why there should only be four possible explanations of development! Can I suggest another? I may be asked. The question is irrelevant. It should be: Is it inconceivable that in the future history of mankind any fifth alternative will ever be put forward? And to this there is surely only one imaginable answer: It is *not* inconceivable.

In view of the hopeless instability of the foundations of Driesch's argument, it would be a waste of time to insist on the inadequacy of his refutations of the three alternatives so summarily rejected. Let us move on to the fourth explanation, "entelechy," which is left victorious in the field. I have called it an explanation: though, so far from explaining anything, it appears to me far more mystifying than the original problem.

Driesch defines it: he says it is an "intensive manifoldness." But since he omits to mention what an "intensive manifoldness" is, we do not seem to be much advanced by the definition; I shall therefore use the word "entelechy" as being shorter and not more incomprehensible than its definition. But I wish to ask what we have learned by this explanation. At the outset we started in ignorance of the etiological factors in development; we finish in an ignorance precisely as dense as that in which we started. We have invoked entelechy but it is no more than a rather pretentious name for our ignorance. Biologists who talk about entelechy are animated by the same spirit that led savage races to ascribe all unexplained phenomena to the act of gods. To say that an event is caused by a god is not in the least an explanation of the event; for our knowledge is then no greater than if we said we did not know how the event was caused: nor is our knowledge of development any greater when we talk about entelechy, about "intensive manifoldness" or about "a true element of nature."

And yet this doctrine, if it be not over-full of meaning, is none the less a dangerous one. The three rejected possibilities of Driesch were mechanistic explanations: the one survivor, "entelechy," is vitalistic. Since it is no more than a name for the unknown truth that we seek, I see no reason why it should be ranked as vitalistic. But it is so: that is the connotation attached to it. Driesch, then, explains morphogenesis by reference to certain known material factors acting in conjunction with a known vitalistic factor called entelechy. He differs from other biologists in that they regard morphogenesis as due to the operation of certain known material factors acting in conjunction with other material factors not yet known nor furnished with a name. It would be interesting to hear how the theory of cancer would be expressed in terms of entelechy. Cancer is now regarded as due to the failure of the organising power in an individual; that is, to the failure of entelechy. Certain cells break loose from the control of the organism and proceed to multiply riotously on their own account, without the slightest reference to the needs of the organism to which they are subjected in a healthy state. Why they should thus break loose is hitherto quite unexplained. But the conception of entelechy here calls up a horrible nightmare. If individual development or morphogenesis be due to

be a machine. For many of our human machines produce the most widely different effects from closely similar stimuli. A little button is pressed and a tiny electric bell may ring or a 20,000 tonner may be launched into the sea or a shock may cause the death of a battalion. So also we frequently produce the same result from widely different stimuli.

But let us consider this question more closely: let us take Driesch's sentence and trace its physiological effects so far as our knowledge extends. The phrase "my mother is seriously ill" first impinges upon the organism in the form of aerial vibrations: it causes a certain specific motion of the molecules of air which happen to be in contiguity with the tympanum. The outer membrane is thus caused to vibrate and transmits vibrations to the three auditory bones; these act as light levers and the vibrations which are carried along them cause, so to speak, a tapping at the *fenestra ovalis* in the inner wall of the tympanum. The fenestra, thus agitated, sets in motion the fluid which bathes it on the inner side. The waves ensuing in that fluid are propagated into the cochlea, pass through the membrane of Reissner, then into more fluid, whence they reach the basilar membrane. Here they produce an excitement of the sensory hair-cells which gives rise to currents in the auditory nerve. From the auditory nerve the currents are carried away down the cochlear branch by several relays to the posterior quadrigeminal and internal geniculate bodies, whence fibres pass on again to the cerebral cortex.

Now I wish to point out that the whole process is *proved* to be mechanical, so far as our laboratory methods enable us to follow it. That difference between "mother" and "brother" is in the first place represented by a different mode of molecular vibration in the outer air. It is represented by a different mode of vibration of the outer membrane of the tympanum, of the auditory bones, of the inner membrane and of the fluids and membranes of the cochlea. The nervous elements distributed to the cochlea are so excessively numerous that they too record the difference, which is thence carried into the brain. So far, the whole process is known beyond question to be mechanical: the machine to be one of almost incredible delicacy and complexity.

But however delicate and complex the auditory apparatus may be, it is infinitely exceeded by the delicacy and complexity of the brain into which the stimulus is carried. Physiologists

can trace the stimulus from the outer air to the auditory nerve : but the infinite complexity which characterises the various nuclei and nervous bodies to which it proceeds they cannot yet trace. How then can we allow Driesch to make the *a priori* assertion that no mechanism could account for the variations in reactions to similar stimuli ? We have traced the stimulus with a variety of changes of form through the auditory *machine*. We lose sight of its path only at the point where the *machine* becomes so excessively complex, the paths of conduction so infinite in number, that its progress can be traced no further. And yet because the ultimate reaction is liable to extreme variation with respect to the stimulus, we are asked to believe that a mechanical procedure is impossible !

We may not know how a watch works : but we do not thereupon deny that it is a machine (though savages do, by the way : they think it is alive and the vitalists among them would no doubt explain its action as due to a " horologic force "). I contend that Driesch has not produced an atom of evidence in support of his opinion that physical mechanism is inadequate to account for the different mental associations set up by mechanically similar stimuli. The original external stimulus in the form of molecular vibrations of the air is transformed by the auditory machine into vibrations of smaller amplitude and greater intensity, in order to be transformable again into nerve currents. Driesch might well say that such a transformation was inexplicable on mechanical principles, had not the actual mechanism been discovered. But if this machine be complex, its complexity is as nothing in comparison with that of the brain where the effects of the stimulus operate. In short, the appearances are so strongly in favour of a mechanical action, that it would be difficult to imagine any other hypothesis.

It becomes possible to account in part for Driesch's difficulty in believing in a mechanical action, if we note that he already begs the whole question by a false definition of a machine. He says : " Does it not contradict the very concept of a ' machine,' i.e. *a typical arrangement of parts built up for special purposes*, to suppose that it originates by contingencies from without ? " His argument, as I understand it, is that cerebral reactions to stimuli are regulated largely by the previous stimuli or " experience " of the brain and that, since this is a matter of chance and since machines are things of definite purpose, cerebral action cannot

be mechanical. To which, of course, the reply is that machines are not necessarily "built up for special purposes": that on the contrary their essential action is in transforming energy or transmitting power: and that they would be just as much machines if that transformation had no purpose whatever. What Driesch does is to define a "machine" in terms which exclude the brain from his definition: and then to argue from these premisses that the brain is not a machine. Well, no mechanist ever said it was, in the sense defined by Driesch! All they have affirmed is that physical and chemical laws alone are in operation: and that has nothing to do with any fancy conceptions of the "purpose" of the machine.

Driesch's *Science of the Organism* is succeeded by his *Philosophy of the Organism*. Vitalism and entelechy having been established on a firm basis by scientific methods, a similar result is achieved by metaphysical methods. I shall spare my reader any account of this part of the work, firstly because (like all metaphysics) it is indescribably dull, secondly because it appears to me loaded with logical fallacies, thirdly because however immaculate the metaphysics might be, however triumphant its proofs might appear, I should not think of believing or attaching the slightest weight to any conclusion that might be reached. The metaphysicians, like the theologians, have had their say. Nothing in the world has ever been discovered by metaphysical methods. No metaphysical "truth" has ever been found in all the thousands of years it has been sought. Moreover anything appears to be susceptible of "proof" by metaphysics. Hegel proved, as we know, that everything is the contrary of what it is. I am credibly informed that certain modern philosophers are of opinion that the part is (or may be) greater than the whole. I am therefore by no means astonished to learn that entelechy rests upon a firm metaphysical basis: and I am content to let it rest there undisturbed. I notice only that Driesch proposes to "establish vitalism" from "the organisation of the Ego": that he has recourse to odd-sounding things like "psychoids" to help him; that during the process it transpires that every man has not merely one entelechy but a whole army of entelechies—a hierarchy of entelechies ranged in authority one above the other; and that ultimately we meet with what I am inclined to call the audacious statement that "life is explained": explained by psychoids and entelechies!

Personally I greatly prefer the Bible explanation. Driesch finally states that there are "three windows into the absolute": the thou, the ego and the it. I fear most people will find these windows too thickly glazed to help them much. For myself I confess I was completely puzzled as to what an "it" might be: and was not greatly enlightened by the definition "the character of givenness." But I merely mention these fatuities to provide an example of what we may be reduced to, if we begin by believing in vitalism.

Driesch is good enough to describe the opinions of those who differ from him as "materialistic dogmatism" and adds in a lofty manner that *he* has "nothing to do with dogmatism of *any* kind." I fear this very superior attitude is not justified by the remainder of the work. By "dogmatism" is usually meant the arrogant expression of an unsupported assertion. Surely then it cannot be applied to physiological mechanism—which is held by the immense majority of physiologists, in contradiction to the wholly unsupported assertion of vitalism. It is clear that Driesch uses the term "dogmatism" as a conveniently stinking carcase to fling at opponents. Indeed, in scarcely any branch of natural science is it possible to express firm belief in some ascertained truth without being called a dogmatist: and that too by people who are prepared at any moment to believe in any rubbish that comes along, without a particle of evidence; moreover, to cherish the idea with that bigoted and cursed obstinacy that is commonly found in alliance with extreme ignorance.

To sum up, we find that the non-metaphysical arguments against mechanism amount to this: "We cannot conceive how mechanical forces could work such a result: therefore they cannot: therefore vitalism is true." That is the entire substance of Driesch's three proofs of vitalism. It is useless for me to insist further on the fact that our inability to understand how a process works is no argument in favour of its incapacity to work. It is useless for me to name such discoveries as wireless telegraphy or Röntgen rays, in evidence of the fact that physical means may produce results that were a short time previously held to be wholly impossible. And it is useless for this reason: that any one who does not instantly perceive the futility of this kind of logic is not likely to be converted by the most *frappant* examples of its failure.

THE VIEWS OF SIR OLIVER LODGE

Not by me at least! Let me therefore pass on to the *dictum* of one who is commonly classed as a friend of vitalism, Sir Oliver Lodge. In a critical article upon the views of Prof. Schäfer, published in the *Contemporary Review* for October, Sir Oliver makes a trenchant protest against founding positive doctrines upon nescience or upon any kind of negation. The vitalist position, as I have endeavoured to point out, is founded mainly (with Driesch entirely) on the negative position that we cannot imagine how mechanism could work. Sir Oliver is thinking mainly of theologians; but his criticism is equally cogent against vitalists and the reason which he gives is equally applicable in the two cases. "Theologians," he says, "have probably learnt by this time that their central tenets should not be founded, even partially, upon nescience or upon negations of any kind; lest the placid progress of positive knowledge should once more undermine their position and another discovery have to be scouted with alarmed and violent anathemas."

But Sir Oliver, notwithstanding his admirable criticism of the chief error in vitalist logic, is himself regrettably disposed to explain away difficulties by the manufacture of metaphysical entities. Criticising Prof. Schäfer he says, "He realises his limitations and definitely excludes the word 'soul' from his consideration; thus proving himself to be in that respect not only scientific, in the narrow sense, but genuinely philosophic." I assume that Prof. Schäfer excluded the "soul" from his consideration because he had other things more interesting to talk about; but it is very difficult to see how he is to be praised for any special scientific virtue in choosing (as he was entitled to do) those other subjects. Sir Oliver's suggestion is, of course, that the "soul" is the concern of metaphysics and not of science; or if of science, not of biology but of psychology. And in a sense he is right: in the same sense that a rattle is the concern of a baby and not of a grown man. But if he means that there can be any knowledge of "soul" or any statement about it that is outside the domain of natural science, then he is wrong. Science is knowledge organised and systematised; *all* knowledge is of the nature of natural science. There is no knowledge of the nature of metaphysics outside the range of natural science. Hence, if the conception of a "soul" is to be accepted at all, it

would appear to be included rightly in the sphere of biology. Many would claim it for psychology. Now psychology has for a long time past been undergoing a transformation from being a branch of metaphysics to being a branch of science. That transformation is analogous to the process by which chemistry developed from alchemy and astronomy from astrology. But it is as yet very imperfectly emancipated. With certain modern psychologists metaphysical whims have full play; but from another school metaphysics is tolerably successfully driven out. There exists a truly scientific psychology; and be it noted, this is just the so-called "psychology without a soul." When we really got to grips with the attempt to explain the properties of mental states, it was found that the conception of a soul was not of the slightest assistance. Not only did it provide no intelligible explanation of anything but it proved to be an actual impediment to rational discovery; in short, it was driven out altogether. In view of the decease of this venerable ghost, it is difficult to understand why Sir Oliver should be so charmed with Prof. Schäfer's omission to dilate upon it; for piety suggests that a funeral oration would have been appropriate to the occasion.

Sir Oliver, I believe, founds his belief in souls very largely on the phenomena of "psychical research"; which is certainly a fragile foundation. I have no space to go into this discredited sphere at present but I cannot resist drawing attention to a recent article published by Sir Oliver in *Bedrock*. The question is of "cross-correspondences," the cases in which two persons in remote localities are smitten by the same idea at the same moment. Dr. Tuckett, who has given some attention to these matters, came to the conclusion that "the coincidences of thought and expression are sufficiently explained by the natural association of ideas in minds preoccupied with the same themes."

To this Sir Oliver rejoins: "That is not the view to which careful students of this subject have been led. If I entered into detail I might ask him why, for instance, Mrs. Verrall and Mrs. Piper should in February 1907 have both been preoccupied with the theme of a 'laurel wreath' and how Mrs. Piper knew—for some part of her certainly knew—that Mrs. Verrall had been so preoccupied." I agree this is a poser for Dr. Tuckett and I should not be in the least surprised to hear that he broke down completely in the attempt to explain why Mrs. Verrall

and Mrs. Piper were both thinking of laurel wreaths. Perhaps there was a Marathon race on at the time, perhaps there was an article on laurels in the *Daily Mail*, perhaps they had been reading classical poetry—a thousand suggestions occur to me but I fail to see how the correct one would help us. In the history of science we have often been struck with the number of great discoveries which have been made simultaneously by persons independently of one another, showing how the thoughts of individuals are controlled by their times. Newton and Leibnitz both thought of the differential calculus at the same time; Neptune was simultaneously discovered by Adams and Leverrier, natural selection by Darwin and Wallace; Mendeléef and Lothar Meyer both hit upon the periodicity of the properties of elements; the true functions of the semi-circular canals in the ear occurred simultaneously to Mach, Breuer and Crum Brown; in 1904 two independent persons thought of the possibility that ticks might carry the spirillum of relapsing fever; and finally, in 1907 Mrs. Piper and Mrs. Verrall were both thinking of laurel wreaths. Now all these coincidences, except the last-named, are very interesting: a valuable essay might be written on the social factors which have brought about these simultaneous discoveries. But that Mrs. Piper and Mrs. Verrall should both have been thinking of laurel wreaths in February 1907 is neither interesting nor in the least significant nor worth investigating whether it be true. Why should they not both have been thinking of laurel wreaths? They must, I suppose, have been thinking of something or other: the range of possible thoughts is not infinite—indeed with most people it is singularly limited. It is tolerably certain, *a priori*, that large numbers of people must always be thinking of the same thing at the same moment. I cannot see, therefore, why anybody should be in the least disconcerted by the information that Mrs. Piper and Mrs. Verrall were both thinking of laurel wreaths in February 1907. Besides, perhaps they were not; mistakes will occur and it is much more likely that one of these ladies made a mistake in trying to recall the subject of her thoughts than that “psychical cross-correspondences” are true. But, as I have already observed, there is no reason to suppose that there was a mistake; since the fact (if true) bears no relation to the conclusion deduced from it by Sir Oliver.

In his Becquerel Memorial Lecture delivered before the

Chemical Society in October last, Sir Oliver adopts an ingenious method of casting plausibility on the existence of ghosts and spiritual events. He adduces a number of instances of the materialising tendencies of science, with a view to showing that the vague, ethereal conceptions of antiquity have given place to definite material entities, previously unsuspected: his suggestion being that as knowledge grows ghosts and phantoms will also become materialised. He gives a variety of instances of this materialising tendency: such as the recent attempt by Prof. Callendar to resuscitate caloric or the material theory of heat; the substitution of material oxygen for its vague predecessor the "acidifying principle"; the tracing of muscular fatigue to material toxins; the causation of malaria by the bite of a mosquito, "a thing which can be crushed with the fingers"; and so on.

But whatever may be the modern tendency with regard to the kind of entities dealt with by science, there surely can be no questioning the fact that in the case of the phantasmagorical entities of metaphysics the tendency is towards increasing rarefaction and de-materialisation. The phantasms of the early Greek philosophers were eminently materialistic. Democritus conceived of the soul as made up of smooth round particles. Thales traced the origin of the universe to the material substance water. But by the time we have reached Plato, the concepts of philosophy become entirely abstract and non-material. That this is the necessary course of philosophic development, I have attempted to show in my book *Modern Science and the Illusions of Prof. Bergson*: for as science advances, the concrete entities of the early philosophers become ever more subject to criticism. If the soul consist of solid particles, science demands the liberty to measure and weigh them. Thus spiritual existences can only hold their own against advancing knowledge by becoming always more ethereal and intangible. They elude the grasp of science as they are de-materialised in proportion to the vigour of scientific assault.

An exactly parallel development has taken place in modern philosophy—where the concepts are now so abstract that only the specially initiated can understand them. Already many of the old phantasms have been refined out of existence altogether and the rest are fast following. In theology, the same process may be observed. In the middle ages, the soul was a thing of definite human form and shape completely materialised; that the

devil had horns, hoofs and a tail was a belief questioned by few : yet I understand that quite different conceptions now prevail in theological circles. The tendency here again is opposite to that alleged by Sir Oliver : it is all in the direction of extreme de-materialisation. Need I amplify by reference to the materialism of modern beliefs among primitive races?—how they leave holes in their graves for the dead man's soul to fly in and out ; or build mounds over the graves to keep the soul in ; how they will not let their shadow fall upon a river, lest a crocodile should eat it ; how they refuse to tell you their name, lest you should steal it ; how they treat diseases by thrashing the patient and surrounding him with foul odours, in order that the “evil spirit” may find its habitat uncomfortable and take itself off. Surely Sir Oliver has been most unfortunate in having recourse to historical evolution as evidence of the future materialisation of spirits. He appears to have confused two totally different meanings of materialism : crude materialism and scientific materialism. Crude materialism is that which allows the existence of ghosts and spirits but says they are made of matter : it differs only from spiritualism in regard to the kind of substance of which the ghost is made and its greater or less refinement ; scientific materialism, on the other hand, is a very modern growth and has no truck at all with any such creatures. The progress of philosophy is from crude materialism *via* innumerable shades of spiritualism to scientific materialism or monism. The ghost, originally material, cannot face science, with its balances and test-tubes. It becomes ever more shadowy and refined ; as the light of science spreads, it recedes further into dimness and attempts to safeguard itself by ever-increasing vagueness and obscurity ; and when its last lurking-holes are lit up, it fizzles out altogether. In directing our attention to the historical evolution of phantasms, Sir Oliver Lodge greatly injures the cause of his *protégés*. To set against the innumerable ghosts of the past which are extinct, can he mention one—*one only*—which has become materialised and established its existence ?

OTHER VIEWS

I need only mention shortly a few other works recently published on the subject now before us. Eugenio Rignano writes a book for the purpose of “explaining the inheritance of

acquired characters." The theory suggested is very elaborate and would be interesting but for the circumstance that in point of fact acquired characters are not found to be inherited. This of course the author denies. He has undoubtedly spent great pains and labour in presenting the subject to his readers: yet his arguments are the old ones with which all biologists are familiar. I find no new facts brought out: what is new is a certain amount of *a priori* speculation. There has never been any lack of *a priori* justifications for the inheritance of acquired characters. Rignano's seems to me as good as any one else's: though in view of the absence of evidence, this theory-building is rather a waste of time. The author somewhat discredits his judgment by affirming at the beginning of his book that the principal object of biology is a search for the nature of the vital principle.

The next book is one by Mr. L. G. Sarjant, entitled *Is the Mind a Coherer?* Its opening sentence is somewhat startling: "Do you ever go out of your mind, reader?" A perusal of the succeeding pages serves to suggest that the question would be more pertinent if addressed to the author than to the reader. After fifty pages we come to the point: "I ask you, reader, 'Is the mind a coherer?' 'I do not know,' you reply.'" In point of fact, I reply that I do know: but suppose that I profess ignorance, the author goes on to define a coherer, lest his readers should not know what it is. He tells us that it is "an instrument, an effect in which can be produced only and solely declaring itself and fulfilling its purpose as an effect in coherence when it bears witness to that similar effect, in a similar instrument produced, which, howsoever produced, was of it the exciting cause." On the next page he adds that although he may be wrong in his interpretation of science, he is seeking, not to be right or wrong but to be clear. The reader, now armed with exact knowledge as to the nature of coherers, has no further excuse for failing to understand the problem at issue. But alas! I can find no facts in the remainder of the work bearing on the question as to whether the mind is a coherer or not. It has struck me however that the author's purpose is not that of answering the question which he has raised but that he is genuinely anxious to know whether or not the mind is a coherer and has hit upon the present method of obtaining an answer. I

beg, therefore, to inform him that the mind is *not* a coherer : and I pass on to the next book.

In his work *Involution*, Lord Ernest Hamilton takes "a glimpse" at the "cosmic process." That glimpse discloses to him the existence of a new kind of ghost called a "Morion" very similar to Driesch's entelechy. This particular spook appears to me in no wise inferior to those of Driesch, Lodge or Bergson.¹ But I think Lord Ernest has in many instances misinterpreted modern opinions too favourably to his own views. He says that "all humanity is groping for God," which is only true, if at all, in a very metaphorical sense. He further asserts that "all men believe in a God," which is flatly untrue. He thinks that "the history of species is the history of a gradual progressive ascent," which is not the case, whatever meaning we attach to the word "ascent." He says that the doctrine of the "interaction of an outside intelligence with what are known as organisms" is now rapidly gaining favour : and that "the chief reason for this change of attitude is found in the complete failure of all attempted explanations of life on materialistic lines." To this, I can only reply that the belief mentioned is not "rapidly gaining" favour but rapidly losing it. Moreover I am not acquainted with any attempt to explain life on materialistic lines within the last quarter of a century and do not therefore see how they have failed. Those who adopt what Lord Ernest is pleased to call the materialistic view do not attempt to "explain life" as he and his friends do. They are only astonished at the facile slurring over the difficulty : whereby mystics imagine they have explained life, by talking about morions, entelechies or psychoids.

Let it be recognised then that science will never permit an "explanation" founded on the invention of new metaphysical entities. Just as primitive peoples are apt to explain everything they cannot understand by reference to the activities of a god, so there still remains a strong mystical inclination to explain "life" by reference to sundry ghosts and spectres to which

¹ It is noteworthy that Bergson has been appointed President of the Society for Psychical Research for the current year ; he therefore may be looked upon as the official head of the ghost-party. If I have made no mention of him in the present article, it is because in my book *Modern Science and the Illusions of Prof. Bergson* (Longmans) I said what I have to say from a scientific standpoint.

strange names are given—vital force being one of the commonest. Not only is there no evidence whatever for the existence of any such shadowy forms but if there were they would not contribute one particle to an explanation: for such entities are in themselves even more mysterious than the facts they are called in to enlighten. The arguments by which Driesch and the mystics support their views are precisely the same as those by which primitive peoples advocate their gods. And not primitive peoples only but the majority of our own society. They see the trees and the grass growing and all kinds of animals and plants: and they say, how can all this have come to pass without the intervention of God? Just so, Driesch contemplates the unexplained facts of organic development and asks: How can all this have come to pass without the intervention of entelechy? By this method, there need remain no obscurities in all the range of knowledge. For whenever facts are unexplained, it is only necessary to invent a ghost, give it a name and ask the "materialist" how he is going to explain the facts without it. The materialist, on the other hand, will regret the introduction of the new factor. In his view, the ghost, even if established, makes the facts no easier to understand: the mystery becomes ever more hopeless; for the facts alone would be simpler to explain than the facts *plus* the ghost. The materialist will see in all this nothing but the overweening pride of ignorance: a pride so great that it remains confident and unabashed in the infinite regions of the unknown: an ignorance so great as to suppose that the greatest mysteries of the universe may be dissolved by recourse to ethereal "principles" built up by man from among the ghosts and fairies which flit at large through his untrained mind.

THE DANGERS OF SOCIALISTIC LEGISLATION

By CHARLES WALKER, D.Sc., M.R.C.S., L.R.C.P.

No living organism, either animal or plant, is exactly like any other living organism, no matter how near the relationship may be : in a litter of collie pups, no individual is exactly like either of its parents ; nor are two individuals in the same litter exactly alike. This is equally true of similar parts of the same organism : no two leaves of an oak tree are exactly alike. Even when we go down to the units of living matter, the cells, we find that no two cells, even those forming parts of the same tissue or organ, are ever exactly alike. It is then obvious that variability is a common property of all living matter. Another property of living matter, also universal, is that when organisms multiply or produce young they produce organisms like themselves. Thus, in spite of the differences already referred to in the case of a litter of collie pups, the pups will be far more like each other and like their parents than they will be like bulldogs or terriers. This similarity extends to parts of organisms. An oak leaf, though never exactly the same as other oak leaves, is far more like them than are the leaves of any other kind of plant ; a liver cell, though never exactly the same as other liver cells, is incomparably more so than is any other kind of cell.

All this must be so readily realised by any one, even though his knowledge and experience be of the slightest ; it is all so easy of demonstration : that, in drawing attention to it, one risks being classed among the apostles of the obvious. What is perhaps not so obvious is that the evolution of living organisms, animals and plants, is entirely dependent upon these two properties of living matter. It is by the action of the environment upon them that new characters, new species and genera, the almost innumerable varieties of animals and plants now existing in the world, have been produced. Of course, the beginning must have been in some primitive form of living matter of which we have as yet no knowledge but it is easy to

see how the process works by considering individual cases. We will suppose a race of deer to be inhabiting a certain area. Within this area are certain carnivora which prey upon them but the deer can run faster than any of their enemies, who have to depend upon cunning and surprises to catch them; a new species of carnivora, more speedy than any of those already there, migrates into this area: now some individual deer will be able to run faster than the majority—for none is exactly like its fellow—and only the faster will escape and produce young. The next generation of deer will inherit their parents' characters with variations, some towards greater, some towards less speed; but they will vary from a higher mean than the preceding generation: those with favourable variations will survive, those with unfavourable variations will be killed. So things will go on, each generation of deer becoming faster, until a racial mean is reached in the characters involved in the quality of speed which ensures a number of individuals escaping from the new and unfavourable factor in the environment. This racial mean will be kept up by the selection of inborn variations, those which tend to lessen the speed of the animals being eliminated. In this manner, either apparently new characters are produced or existing characters are maintained at a high standard by natural selection. Of course, if the selection be too stringent, a race or species may be entirely exterminated before there has been time for the new characters to be produced, as would have happened had the new carnivora been so numerous and their speed so great, in the case of our deer, that none of the deer had been able to escape.

There is another matter which claims attention before the consideration of the manner in which selection is acting at the present time in the case of civilised man is taken in hand. This is, that as soon as a character ceases to be the subject of selection—in other words, as soon as the environment ceases to be detrimental to those individuals who do not possess it or advantageous to those who do—the character begins to disappear. A good example of this is the blind cave fish, whose ancestors possessed functional eyes, as is shown by the fact that they all possess undeveloped eyes. Being useless in the dark, functional eyes, however, give their possessors no advantage either in obtaining food or in escaping from their enemies or in any other way. Selection having ceased to act in the maintenance of this character

in the cave fish, their power of sight has been lost. Innumerable similar examples are available. Some characters disappear more rapidly than others in the absence of selection ; but there is no need to discuss this point here.

There can be no doubt that mental as well as physical characters are subject to selection by the action of the environment. Take the example of a pointer. Very likely a bulldog might be taught to point but there cannot be the slightest doubt that pointers generally are more easily taught to point than are bulldogs. The efficiency of pointers as pointers is maintained only by the very stringent way in which selection is exercised by the breeder ; their mental characters are just as much subject to selection as the shape of their heads : those animals are most sought after to breed from which have proved themselves to possess the greatest power of displaying the mental characters involved in pointing ; the characters involved in pointing have been produced by selection from a common ancestor of the pointer and the bulldog in comparatively recent times.

Among men, a very good example exists in the case of the Jews. During hundreds of years, they were subjected to most stringent selection—a selection which still continues to operate to some extent in some countries. They were not allowed to carry arms when every man went armed, so that any Jew who showed any signs of combativeness or desire to resist oppression by physical violence must have been eliminated at a very early stage in his career and can have had but little chance either of having or of bringing up children. Before they were dispersed and subjected to this very stringent selection, the Jews were probably the most quarrelsome, bloodthirsty and combative race known to history. I think it would not be going too far to say that they are now among the most peaceful. At the same time the selection to which they were thus subjected has acted upon their mental character in other ways. It was only by the exercise of great intelligence that individuals were able to survive ; the stupid must have been eliminated, only the clever could escape. The result is that the Jews to-day probably possess a higher average of intelligence than any other race in the world ; but only in peaceful pursuits, for we do not hear of great soldiers and sailors among them. I would here emphasise the fact that I am speaking in general terms. I do not mean that there are no combative or quarrelsome Jews but that on the average the race

is remarkably peaceful. I do not mean that there are no stupid Jews but that on the average they rise higher in intellectual pursuits, including the acquisition of riches, than do their Gentile neighbours.

Now to apply all this to the consideration of socialistic views and socialistic legislation. It is clear that the racial standard of any character, mental or physical, must depend entirely upon the stringency of selection to which the bulk of the individuals of the race are subjected with regard to the character. The race which can maintain itself with the least effort will be the least industrious; that which has never been subjected to infection by a certain disease will be incomparably more susceptible to it, if the infection be introduced into its environment, than will a race that has been subjected to infection during many generations. The characters of a race are dependent entirely upon the existing environment and that which has existed in the past, for the environment determines the selection of those variations which give their possessors an advantage over their fellows. In a civilised community, mental qualities are sometimes of greater importance to the individual than physical, within certain very obvious limits. In this country the social conditions, during a long time past, have been such that there has been a constant interchange between the various classes. Unfavourable variations among individuals of a higher class must cause a fall into a lower class, which can only be checked by the occurrence of favourable variations among the offspring; the occurrence of favourable variations will result in the individuals "bettering themselves" and if these favourable variations are inherited by the children, they in their turn will either maintain the position gained by their parents or improve it if the favourable variation have been increased. In cases where property and position are by law inherited entirely or in overwhelming proportion by the eldest son, the process of falling into a lower class, in the case of the occurrence of unfavourable variations, may be to some extent checked but the extent to which this can happen and the number of individuals involved cannot possibly be sufficient to produce an appreciable effect upon the mean of the mental and physical characters of the race. A cursory examination of the family histories of the present members of the House of Peers, the class most protected in

this way, will show how short is the time during which a succession in the male line is maintained. On the other hand, even in the case of the aristocracy, all but the eldest son in each generation are dependent upon the maintenance of a certain standard of efficiency; if unfavourable variations occur and are handed on to the offspring, the individuals forming the off-shoots—one should perhaps rather say the overwhelming majority of the members—of these families must rapidly sink to lower classes in the social scale. In the lowest class the stringency of selection, particularly with regard to physical characters, is very high, as is shown by the very high infant mortality among other things. At the present time it is probably far easier for individuals to rise from the lower classes than was formerly the case and it is just as difficult for individuals to remain in the higher classes and place their children in positions similar to their own.

It must of course be realised that man is the supremely educable animal but this fact has unfortunately so impressed some people who write upon social problems, that the inheritance of mental capacities as distinct from superadded education has been completely overlooked by them. No two boys in the same class in a school will show precisely the same ability in acquiring knowledge or skill. Moreover, one boy will very likely be brilliant in mathematics but hopeless in some other subject; whilst the boy who is good in this other subject may be incapable of attaining to any great efficiency in mathematics. Probably all the class will be capable of attaining to a certain degree of efficiency in any subject but the labour involved in such attainment will vary in every case and some of them will be unable to get beyond a comparatively low standard. The various mental capacities are undoubtedly transmissible from parents to offspring with variations towards increase or decrease.

At the present time the selection of physical characters, particularly among children, is most stringent in the lowest class but it extends to all. In the case of all individuals there is a continual competition by which the least efficient are selected and placed under conditions under which the mortality is highest. Of course, I do not for a moment contend that there are no cases in which an individual, through an unfortunate concatenation of circumstances, is kept in a position

not equivalent to his mental and physical efficiency; but, as a rule, there cannot be any doubt that the possessors of favourable variations rise and that if these variations are handed on to the children they continue the upward movement. The converse is obviously true of unfavourable variations, particularly when they are transmitted to the offspring. One excellent feature of the process, from a biological point of view, is that it is usually slow. An individual does not, as a rule, himself rise directly from the lowest class to a high one and start his children in life there; for a substantial rise, several generations, involving the continuance of favourable variations, are generally necessary. The converse is true with regard to a fall. It is far more probable that a high standard of capacity will recur in the offspring of an individual whose ancestors, during many generations, were of a high standard, though he himself varied unfavourably, than that a high standard of capacity should occur in the offspring of parents whose ancestors, during many generations, had never risen from the lowest class.

Now all the systems of socialism appear to me to aim at mitigating the stringency of selection with regard to capacity. The least that any of them aim at seems to make such provision that every individual shall be able to live under healthy and even comfortable and happy conditions and that all shall have similar opportunities when starting in life. This involves a limitation of selection, particularly in that the children of efficient parents are not placed in a better position from which to start than the children of inefficient parents. Competition is selection in civilised communities and the ideal state of things would be that all individuals who fail in this competition beyond a certain point should be eliminated. On account of the sentimental feelings towards the individual which, in our country, are concomitant with the advance of civilisation, active measures in this direction are almost inconceivable. Hitherto, a passive elimination of the inefficient has gone on to a considerable extent, through the action of bad and insufficient food and bad hygienic conditions generally upon the lowest classes of society. There appears to be no doubt whatever that modern legislation is removing this very necessary form of selection and is giving us no protection in substitution for it. As it is impossible to deal with many points in detail, I will select one or two examples.

Hitherto, unless individuals, particularly those in the lower classes, considered the future to some extent, any unexpected misfortune such as illness or unemployment placed them and their children at a very considerable disadvantage; the probability of having children or of successfully rearing those already existing would be considerably diminished as compared with the case of individuals who had foreseen the possibility of such misfortunes and provided against them. Many of the lower classes did so provide; consequently there was a constant process by which the provident became selected as against the improvident. Now, under the Insurance Act, the improvident are forced to provide to some extent against unfavourable contingencies and the process of selection is seriously interfered with in consequence. The elimination of the improvident and their children is to be prevented as far as the law can manage it. Much the same criticism may be applied to the feeding of school children.

Not so very long ago lunatics were treated as criminals: the treatment they received was such that recovery was wellnigh impossible and the production of children was prevented. All kinds of lunacy are not transmissible from parents to offspring; but most are and idiocy certainly is. The effect of modern legislation with regard to lunatics and idiots is such that whilst it is now made as difficult as possible to keep them under restraint, they are treated in a way to improve their condition and set at liberty upon temporary improvement; they therefore gain a renewed opportunity of perpetuating their kind. The result is an increase in the number of lunatics, which increase is progressing at such a rate that the public must inevitably be frightened at no distant date; the Government of the day will then be forced to legislate afresh but it is to be feared that sentiment will again intervene to prevent the introduction of satisfactory measures.

One of the greatest dangers in the immediate future appears to lie in thoughtless and sentimental legislation dealing with disease. In some cases, there can be but little doubt that legislation might do much towards the elimination of particular diseases. In other cases it is almost certain that legislative interference will be attended with a vast amount of harm and with no possible chance of doing good. Those responsible for this kind of legislation often appear to be either ignorant of the complicated nature of the problems with which they are dealing

or to come to a conclusion without having devoted any consideration at all to the consequences of their endeavours.

An amount of injury to the race which it is difficult to overestimate is likely to follow upon the recent legislation concerning tuberculosis. The people of Northern Europe have been subjected to a very stringent selection as regards tuberculosis during several thousand years; the selective process has become more stringent of late years in proportion to the increase in the town population. At the present time it is so stringent that, probably every individual in Northern Europe, living in a town or even in a village, is infected with tubercle many times during life. I do not mean merely that the tubercle bacillus gains access to his body and is immediately eliminated but that it becomes established therein and multiplies, being eliminated only after some time. The evidence that this happens is overwhelming. Thus Ribbert has published the records of 5000 consecutive post-mortem examinations of cases that died in general hospitals.¹ Traces of tuberculosis were found in every one of these cases. In all similar records of which I know, the lowest percentage of cases in which traces of tuberculosis have been detected is seventy-five. It has also been shown that very frequently the signs that are met with of tubercular disease of the lungs of long standing indicate very considerable and extensive damage and destruction of tissue, not slight infection;² yet such individuals have recovered from the disease and this has had no permanent effect upon their health. Now it is well known that when the tuberculosis bacillus is introduced among a race which has had no previous experience of the disease many individuals contract the disease and die of it rapidly under conditions which would bring about a cure in susceptible European patients.

The explanation of this fact is quite simple. The relative immunity of the European has been brought about by a process precisely similar to that described in the case of the deer and the carnivora. When the tubercle bacillus first appears, the different individuals of the race will differ in their susceptibility to its ravages, just as they differ in other characters. The least susceptible will have an advantage over the more susceptible and will have a greater chance of producing and rearing children. Taking the average resistance of the race originally as 0, some

¹ Quoted by W. Osler, *Principles and Practice of Medicine*, 1904.

² Brouardel, *Trans. British Congress on Tuberculosis*, vol. i. 1902.

of the individuals will have a greater resistance than the average and these may be classed as + 1. Others will have less—these will be - 1; others will be 0. In the next generation, however, more children will have been produced and reared by the + 1 individuals. Offspring inherit their parents' characters with variations but this second generation will vary from a new mean, + 1; some individuals will vary towards a greater, some towards a less and some will inherit the character in the same degree as their parents. We shall, therefore, have a generation of individuals consisting of 0, + 1 and + 2. Obviously, the + 2 class will have the greatest chance of surviving and rearing children, so the next generation will vary again from a higher mean, + 2. This process must continue as long as the tubercle bacillus continues in the environment, until a very high degree of immunity is attained by the race. Of course, variations away from the average of racial immunity must continue to appear but the standard of the race is maintained because these unfavourable variations are eliminated. There is undoubted evidence that tuberculosis existed in Egypt about 5000 years ago,¹ so it is practically certain that it occurred also in countries further north which had communication with Egypt,² at any rate indirectly, where the conditions are as favourable to the tubercle bacillus as they are unfavourable in Egypt. Northern Europeans have therefore been subjected to selection during several thousand years; hence comes their comparative immunity. The effect of recent legislation must certainly be to lower the standard of immunity of the race, as the susceptible individuals are to be taken in hand wholesale and kept alive to breed children, who will vary in their immunity from a lower mean than that from which their parents varied.

It is unfortunately inconceivable that the tubercle bacillus can be eliminated altogether. It is able to survive in a dried-up condition during a very considerable period of time and it is probable that the inhalation of dried tubercle bacilli is a common cause of pulmonary tuberculosis in the case of susceptible individuals. Besides this, tuberculosis is probably as common in cattle and perhaps other animals as it is in man. If, even in spite of this wide distribution, the bacillus were eliminated in

¹ G. Elliot Smith and Wood Jones, *Archæological Survey of Nubia*, Report for 1907-8, vol. ii. (Cairo, 1910).

² G. Elliot Smith, *op. cit.*

the British Isles, it is inconceivable that its introduction from abroad could be prevented. Therefore, if susceptible individuals are kept alive and allowed to breed in large numbers we must expect serious ravages in the future, when the racial standard has been lowered and a temporary concatenation of circumstances favours the infection of a large number of individuals at the same time. The nation whose racial standard is thus lowered by legislative interference must eventually be supplanted by an invading race which has continued to exist under conditions of stringent selection. Under invasion I intend to include simply the immigration of immune individuals.

The case of typhoid fever belongs to the class in which legislative interference might bring about the suppression of the disease. The micro-organism can only continue to live under a comparatively limited set of conditions, which may conceivably be eliminated.

Smallpox is in a rather different category. In this case, the individual may be rendered, by artificial means, entirely or to a large extent immune to the disease during the whole of his life and that very easily. Unfortunately this is, the only case in which a comparatively permanent immunity to a particular disease can be produced early in life. The original legislation enforcing vaccination upon every individual was wise. The modern vice of sentimentality, which attaches so high a value to the feelings of the individual but is regardless of the interests of the community at large, has allowed any one who wishes to refuse to allow his children to be vaccinated. We have already experienced the consequences of this evasion in Gloucester and other places and are likely to have further and more serious demonstrations of the folly of our latter-day legislation in the near future.

The legalisation and protection of trades unions are equally disastrous in so far as these associations tend to equalise the inferior and the superior workman. Anything that places the unskilful and idle on the same footing as the skilful, hard-working and steady man with regard to wages must tend to eliminate to a great extent the selection of these desirable qualities. The standard of efficiency to begin with may be that of the average man but as there is no advantage to be gained in being above the average, competition is interfered with; selection, in fact, ceases to operate and though there is nothing in the environ-

ment which can raise the average, all the factors tending to make it fall remain; fall, therefore, it certainly will. Many employers of skilled labour who are competent judges upon this point are of opinion that the average has already fallen and is continuing to fall. It does not appear that the so-called leaders among the working classes are amongst the most diligent and skilful at their various trades. The best men still have a practical certainty of employment, at any rate in trades requiring skilled labour; many that I have met feel that they might be much better off in the absence of union scales of wages, as doubtless they would. But when the employer of skilled labour is obliged to get a large quantity of work done for a certain sum—being limited by the price he, in his turn, can obtain—he is obliged also to pay a minimum if not a uniform wage; the obvious result must be that the least efficient man is paid more than he is worth, the most efficient man less. From the point of view of selection and the maintenance of a high standard of efficiency in the race, there cannot be the slightest doubt that it would be far better for the inefficient to be sweated and the efficient to be paid too much. From the point of view of present-day sentiment, the poor inefficient is to be saved from suffering and even from discomfort at any cost. The cost must obviously be paid in some form or other by the efficient and the ultimate result must certainly be the lowering of the general efficiency of the nation.

It is probable that the highest standard of efficiency is in the professional and upper middle class generally, where selection acts most quickly. Unfortunately, this class is the busiest; moreover, it is not often subject to the efforts of the agitator and is not combined in a political sense. Men of this class confine their attention to a great extent to their work and though in them lies the overwhelming proportion of the mental capacity of the nation, they play but a small part proportionately in the government. Unfortunately, also, the sentimentality of the age is as strongly developed in this class as in any other and apparently no consideration is given by them to the ultimate effects of indulgence in this weakness. Failures, including criminals, as well as the children of failures are more and more protected and kept alive to breed more failures. Failure in competition for a livelihood means, in the overwhelming majority of cases, that the individuals are so much below the average in

mental and physical capacity that it is necessary, if the racial standard is to be maintained, that they should be eliminated or at any rate should not produce children. There are exceptions but it is better that the exceptions should suffer than that the race should fail. Our intellectual and physical characters have been produced by the action of natural selection upon inborn variations. Unfavourable variations have been eliminated. Elimination is necessarily unpleasant to the eliminated and whilst to-day, horrified at the cruelty of the process going on in our midst, we are preventing it by all the means in our power, we are providing nothing to take its place. In the absence of selection, characters must disappear; inborn mental capacities of every kind are just as much heritable as are arms and legs. Any social legislation, therefore, which interferes with the unpleasant process of the elimination of the unfit must result in the diminution, if not in the disappearance, of those characters which are so eminently necessary in the competition between different nations—a competition that cannot be abolished by anything that a single or even several nations can do.

THE DETECTION OF PREGNANCY

UNDER normal conditions, the substances entering the blood stream, apart from the fats, are presumably the simple products fashioned during digestion from the complex materials taken as food; they are either rebuilt into various tissues or gradually utilised as sources of energy; and in the latter case are finally resolved almost entirely into carbon dioxide, ammonia and water.

Under the conditions of disease, more complex substances may enter into circulation or the blood may become more or less infected with micro-organisms. It is all-important to determine what are the agencies at the disposal of the animal organism whereby such intrusions are countered and rendered ineffective.

The body cells generally undoubtedly contain enzymes capable of acting on albuminous materials, on carbohydrates and on fats; these are set free when the cells are subjected to the action of hormones or to mechanical disruption. But fresh blood plasma and serum, in the case of most animals, appear to be without hydrolytic power.

Prof. Abderhalden, who has been an active worker in this field of late years, has recently published an interesting account of his views and experiments in book form.¹

Observations have been carried out by injecting various hydrolytes and after an interval observing the action of the blood plasma or serum on the hydrolyte, normal plasma or serum having been found to be without action. To ascertain whether an effect had been produced, serum from the treated animal was mixed with a solution of the hydrolyte and the change in optical rotatory power which took place over an interval of several hours was followed by means of the polarimeter. A specially constructed short tube was used in carrying out the observations, so as to permit of the use of small quantities of material.

¹ *Schutzfermente des tierischen Organismus. Ein Beitrag zur Kenntnis der Abwehrmassregeln des tierischen Organismus gegen körper-, blut- und zelfremde Stoffe.* Von Emil Abderhalden. (Berlin: Julius Springer, 1912 [pp. xii + 110.]

To quote a particular experiment. On each of two successive days, five grams of cane sugar was injected intravenously into a dog and on the third day blood was withdrawn from the animal and tested. In making the test, one cubic centimetre of serum was mixed with one cubic centimetre of a 10 per cent. solution of cane sugar and 5 of physiological saline. The initial rotatory power of the mixture was $+ 0^{\circ}45$; observations made at intervals during forty-five hours showed a steady fall in the rotatory power, which finally became $- 0^{\circ}5$. In a second experiment, in which ten cubic centimetres of a 5 per cent. solution of sugar was injected intravenously, the serum was found to be active fifteen minutes after the injection. In the case of a dog which had only received a single injection, the serum was slightly active fourteen days afterwards; whilst that from one which had received two injections subcutaneously was strongly active nineteen days afterwards.

Similar results had been obtained previously by Weinland, who had also made the remarkable observation that when either milk sugar or soluble starch is substituted for cane sugar, the blood acquires the power of hydrolysing cane sugar but that these substances apparently do not provoke the appearance of corresponding enzymes. Raffinose, a more complex sugar than cane sugar, seems to be without action.

Similar observations have been made with albuminous substances and peptones—but the same remarkable result comes out in these experiments: the response being a perfectly general one, not specific, the blood plasma acquiring the power of hydrolysing substances generally of the class to which the hydrolyte used belongs, not this hydrolyte alone.

In view of the statement that it is possible to detect invertase in blood plasma fifteen minutes after the subcutaneous injection of cane sugar, Abderhalden's further statement is remarkable that when albuminous materials are injected, it is often many days before proteoclastic activity is fully developed.

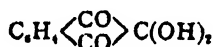
It is obvious that much is yet to be learnt before it will be possible to give a consistent explanation of the observations, the evidence at present being far from decisive.

Perhaps the most interesting outcome of the work is that relating to the peculiar condition of the blood in pregnancy. It is well known that during this period chorion cells pass from

the ovum into the blood, of which they are not normal constituents. Presumably means exist whereby these are destroyed. Abderhalden finds that normal blood plasma and serum are without action on the peptones prepared from human placenta but that they are hydrolysed by blood plasma from pregnant women and even from pregnant animals; and in this case the effect is specific, little or no action taking place either with ordinary albuminous materials or with peptones prepared from them. The effect is noticeable from the first month of pregnancy to the close but disappears within eight days after delivery.

To ascertain whether action had taken place, Abderhalden originally used either the optical method or subjected the crude mixture of peptone and plasma to diffusion and applied the well-known biuret test to the diffusate. Recently an important new test has been introduced.

In 1910, Dr. S. Ruhemann was led to prepare a substance to which he gave the name *triketohydrindene hydrate*, a compound represented by the formula—



This compound behaves in a most characteristic manner with amino-compounds and when warmed with amino-acids gives rise to coloured products: the test is an extraordinarily sensitive one, so that if a solution containing a very minute quantity of the keto-compound and of an amino-acid (glycine, alanine, leucine, tyrosine, etc.) be warmed, a blue colour rapidly makes its appearance.¹

The proportion of amino-acids present in normal blood is so minute that they cannot be detected in it by means of this test but in pregnancy they are at once apparent. To apply the test, a little of the serum is first subjected to diffusion, as peptones and proteins also give the blue colour; the diffusate is then warmed with a little of the keto-compound. No other condition has been discovered in which the test gives positive results—so that it is of great diagnostic value.

It has been the fashion of late—especially among physicists—to decry the work of chemists and to stigmatise them as mere

¹ *Chem. Soc. Trans.*, 1910, 2025.

preparation-makers. Physicists unfortunately too often have no proper knowledge even of the elements of chemistry let alone of the higher walks of organic chemistry, so that they are unable to appreciate the methods of the chemist and the progress that is being made by his persistent efforts to discover new paths by which the infinitely difficult problems of physiological chemistry can be approached. We can put up with a very large amount of dull work, if occasionally a discovery be made so useful as that under notice is likely to prove. If special colour tests applicable to particular diseases—syphilis and tuberculosis, for example—could be devised, they would be of the greatest value; as it is more than probable that different diseases are attended with chemical changes special to each disease, it is to be expected that simple tests may ultimately be found that will at least facilitate diagnosis, if they do not make it certain.

THE BLEACHING OF FLOUR

SATISFACTORY as is the increasing amount of interest taken by legislative bodies in the purity of manufactured foods, it is to be regretted that expert and scientific advice is not more often sought before framing restrictive regulations; in consequence, measures not infrequently become law which are either unworkable in practice or impose a grievous restriction on the honest manufacturer; and even, as in the case of milk and butter, legally debase what was previously, in most cases, an article of high quality. Our own Local Government Board should be free from this criticism, since it is at pains to anticipate legislation by reports prepared either by its own or by co-opted experts. Admirable, however, as this plan should be in theory, it has proved somewhat disappointing in practice. There is an increasing tendency to take certain conclusions as proved, even against the weight of scientific evidence and contrary to the canons of scientific research. Such action can only be regarded as a deplorable subversion of intelligence; taken in conjunction with the present-day exploitation of science by company promoters and by advertisers of proprietary foods and medicines, it is undoubtedly producing an adverse effect on the public attitude towards science.

The Local Government Board has recently issued the third of a series of Reports¹ relating to the bleaching of flour, in which experiments are described that have been made in the Laboratories of the Board. In this pamphlet certain results are described, almost without comment, which are directly at variance with the conclusions arrived at in former Reports on the same subject, so that it seems desirable to criticise the pronouncements in detail.

In the former Reports (see SCIENCE PROGRESS, April and October 1911) dogmatic statements were made as to the injurious effects of bleaching flour—we are now favoured with an account of experiments made to determine what bleaching actually does to flour and the influence it has on the baking qualities.

The immediate effect of bleaching flour is to destroy the colouring matter. It has been suggested by Wesener that the pigment present is identical with Carrotene, a yellow unsaturated hydrocarbon which recent researches, more particularly those of Willstätter, have shown to be widely distributed in plants. To confirm this suggestion, Dr. Monier-Williams has compared the absorption spectrum of carrotene with that of a flour extract and finds the two to be identical. It appears that elaborate spectroscopic apparatus was obtained and much time spent in research before this conclusion was reached—would it not have been easier to have made use of the facilities offered by one or other of the many University laboratories in which such apparatus has long been installed? Pure carrotene was prepared from other sources and the effect produced on it by nitrogen peroxide compared with that of oxygen. Nitrogen peroxide bleaches carrotene, products containing nitrogen being formed. When exposed to the atmosphere, carrotene becomes lighter in colour and absorbs oxygen; no nitrite could be detected in a sample (0·2 gramme) after such exposure. It is assumed that the action of the two gases gives rise to different products and hence that their action on flour is also entirely dissimilar. It is a far step to take from the first to the second of these statements on such slender evidence.

¹ Report to the Local Government Board on the nature of the colouring matter of flour and its relation to processes of natural and artificial bleaching. By Dr. G. W. Monier-Williams. Food Reports, No. 19. (London: Wyman & Sons. Price 3d.)

In his previous Report, Dr. Monier-Williams made sweeping assertions regarding the injurious effect of bleaching on flour. He now produces experimental evidence to show that when flour is exposed to the atmosphere, stored in calico bags under conditions very similar to those which prevail in the retail trade, it absorbs a minute proportion of nitrite from the air. The quantity so absorbed was an amount equivalent to 1·2 parts of sodium nitrite per million, whereas commercially bleached flour of the type manufactured in London did not contain more than 1·6 parts. The difference is so small that it is difficult to avoid the conclusion that the same ultimate result is attained whether the flour be bleached rapidly by artificial means or slowly by the gradual absorption of nitrogen peroxide from the atmosphere. This contention is dismissed in the Report because a sample of pure carotene did not absorb nitrite from the air. The power flour, starch and similar materials have of absorbing all sorts of things from the surrounding atmosphere is entirely ignored.

Not only does ordinary bleached flour absorb no further nitrogen peroxide on exposure but highly bleached flour loses most of its nitrite on prolonged storage and it is admitted that "under ordinary conditions of storage" there is "an approximate figure towards which the nitrite content of all samples, whether highly bleached or unbleached, will eventually converge."

Hitherto the Local Government Board experts have been silent as to the effect of bleaching on the baking qualities of flour, though this is in reality the crux of the whole position. No baker would use a flour if it had any effect on the quality of his bread: the public are greater adepts at noticing such niceties than is generally supposed. The services of Mr. Kirkland, of the Borough Polytechnic, have now been called in to make bread from flour subjected to different degrees of bleaching far in excess of the commercial quantities. He reports that, with the exception of the loaf from a flour containing 75 parts of sodium nitrite per million, all the loaves were of excellent quality and had no taste or smell!

Comment should be unnecessary. Dr. Monier-Williams himself shows that his earlier conclusions were entirely illogical and it is difficult to understand the attitude he took up in his former report.

The whole question has been the subject of an important legal case during 1912 in connexion with a prosecution for flour

bleaching. The experts for the prosecution concerned themselves with the effects of large amounts of nitrite ; the defence was directed to flours as actually treated by a bleaching plant. The judge found that commercially bleached flour cannot be proved to be different from unbleached flour and that the result of commercial bleaching was merely to alter the colour without altering the nature, substance and quality of the flour so as to render it a different article !

A remarkable aspect of the case was the attitude taken up by some of the experts in deducing the behaviour of very small quantities of a substance from what is known of the action of large quantities. This method has been carried to exaggeration by Dr. Wiley in America but that it is entirely fallacious few scientific men will deny. In the first place, it ignores entirely all possibility of selective action, such as is bound to occur when an active agent is brought into contact with so complex a mixture as flour ; secondly, scientific literature is full of well-authenticated instances of the beneficial action of traces of substances which in larger quantities act prejudicially ; much has been done of late to put our knowledge of the mode of action of these small quantities on a firm basis : it is therefore disconcerting to find scientists of eminence adopting an attitude in the witness-box so much at variance with proved fact as appears to have been the case in the trial referred to.

In view of the foregoing considerations, it is obvious that there is grave danger in basing action affecting interests so great as those of the milling trade on the partial opinions of persons who necessarily have only a limited knowledge and experience of the practical side of the question at issue.

RADIOACTIVITY VISUALISED¹

By C. T. R. WILSON, M.A., F.R.S.

THE phenomena of radioactivity are known to be due to the ejection from the atoms of the radioactive elements of two kinds of particles which travel with enormous velocities: (1) the alpha-particle, which is a positively charged helium atom having a mass four times that of the hydrogen atom; (2) the beta-particle, which carries a negative charge only half as large as the positive charge of the alpha-particle and has a mass less than the 1700th part of the hydrogen atom.

The velocity of the fastest beta-particles approaches very nearly to that of light, that of the alpha-particles being considerably less but still exceeding 10,000 miles a second.

By the action of Röntgen and other radiations, we can cause electrons or corpuscles which are identical with the beta-particles to be expelled from the atoms of any element with velocities comparable with those with which the alpha-particles are ejected from radium.

The methods which have been used hitherto in the study of the paths of these projectiles and of the effects produced by them in their flight have been somewhat indirect. The actual paths of individual particles have not been observed; it has been necessary to investigate the combined effects of a large number of particles.

It is true it has been found possible by two different methods to detect effects arising from the action of a single alpha-particle. Thus Rutherford introduced a method in which effects due to the ions set free along the path of a single alpha-particle could be detected by an electrometer; again in the Crookes spintharoscope each alpha-particle causes a starlike point of light to flash forth momentarily where it strikes the prepared screen. But it has not been found possible by such methods to detect effects arising from a single beta-particle.

¹ A lecture delivered at the Royal Institution on the evening of Friday, February 28, 1913.

It is plain that a great advance would be made if it were possible to induce each alpha- or beta-particle to leave a visible trail behind it along its whole course and to photograph this trail. This is what is accomplished by the method now described.

Each alpha- or beta-particle, in the course of its flight through a gas like air, traverses large numbers of the atoms of the gas. According to modern theories, such as those developed by Sir J. J. Thomson and Rutherford, each atom may be regarded as a sort of miniature solar system in which the planets are represented by negatively charged corpuscles or electrons; the forces with which we are concerned being of course electrical and not gravitational. When either an alpha- or a beta-particle passes near one of the members of the system, there are forces tending to deviate the flying particle from its otherwise straight course and to cause disturbances in the path of the planetary electron; these may be violent enough to cause the electron to escape from the system. An electron thus set free will become attached finally to some other atomic system, which thus acquires a negative charge, whilst the atom which has lost an electron has been left with an excess of positive electricity. We thus get positively and negatively charged atoms or ions.

Now a method of making visible the individual ions has long been available. Molecules of water or of other vapours attach themselves more readily to ions than to uncharged atoms or molecules. Thus, in the absence of other nuclei on which vapour can condense more readily, such as those called dust particles by Aitken, it is possible to arrange that every free ion shall act as a nucleus and cause the condensation of water vapour, whilst none condenses elsewhere. Each invisible ion may thus be converted into a visible water drop. The supersaturated condition necessary in order that water vapour may condense on the ions is most conveniently produced by the sudden expansion of moist air.

The advance which I have recently succeeded in making in the condensation method of studying ionisation is this. The ions are now captured and converted into visible water drops in the positions which they occupied immediately after their liberation by the ionising agent; the cloud of drops is then at once photographed. Thus the invisible trail of ions

left behind along the course of any ionising particle is converted into a visible line of cloud of which a photograph is secured. In this way a record is obtained of the path of each projectile by making visible the atomic wreckage it has caused in its passage through the air or other gas. In many cases the individual ions produced along the tracks are visible in the photographs.

In order that undistorted pictures showing the result of the passage of the various rays may be obtained, it is essential that the expansion should be effected without stirring up the gas. This condition is secured by using a wide shallow cloud chamber of which the floor can be made to drop suddenly and so produce the desired increase of volume (fig. x).¹

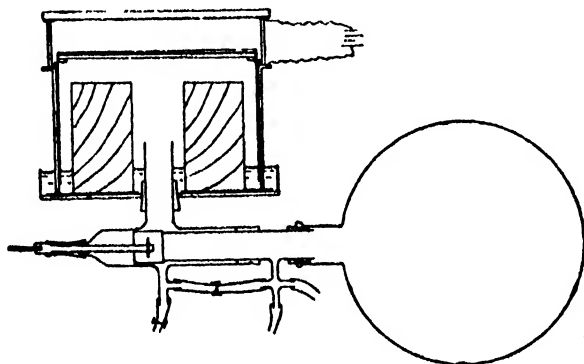


FIG. x.

It is hardly necessary to say that the cloud chamber must be freed from dust particles and all nuclei on which water readily condenses. This is easily done by repeated expansions, each too small to cause condensation on the ions, any cloud formed being always allowed to settle before making another expansion.

The cloud chamber must be free from ions other than those produced by the ionising agent under investigation. Since ions are always being produced even under normal conditions within a closed vessel, it is necessary to maintain an electric field between the top and bottom of the cloud chamber, so that they may be removed as fast as they are produced.

¹ The apparatus is described in the *Proceedings of the Royal Society, A.*, vol. 87 (1912), p. 277.

One very practical point in connexion with the cloud chamber remains to be mentioned. It is necessary that the interior should be maintained in a nearly saturated condition and yet that the roof and walls should be transparent and admit of a clear and undistorted view of the contents. A glass vessel containing moist air soon becomes coated internally with a dew-like deposit of minute drops. This difficulty is completely avoided by covering the inner surface of the glass with a film of gelatine.

The moist gelatine under the plate-glass roof of the cloud chamber forms a conducting film which is connected through a marginal ring of tinfoil with one terminal of a battery of cells, the other terminal being connected to the floor. In this way, a nearly uniform vertical electric field is maintained between the roof and floor of the chamber. The floor is virtually a pool of water made solid by the addition of gelatine and blackened by means of ink so that it forms a dark background for the clouds. It is supported by a glass plate which forms the top of a hollow cylindrical-plunger working in water.

As regards the actual mechanism for causing the sudden drop of the floor of the cloud chamber, it is sufficient to state that the space below the plunger can be put in communication, through wide tubes, with an exhausted chamber by suddenly opening a valve.

In order that the ionising particles should leave sharply defined cloud trails, it is necessary that they should traverse the moist gas immediately after this has been expanded while the water vapour is still supersaturated to an extent considerably exceeding the minimum which is required to cause condensation on the positive ions (which are more difficult to catch than the negative). Under these conditions, the ions lose their mobility and grow into visible drops before they have had time to diffuse appreciably away from the original track of the ionising particle.

If the clouds formed by condensation on the ions are to be photographed, it is necessary to expose them to an instantaneous illumination of great intensity while the camera is in position. The instantaneous illumination is obtained by a Leyden jar discharge, the arrangement being essentially the same as that used by Lord Rayleigh in photographing jets of water and by Worthington in his study of the splash of a drop.



FIG. 1



FIG. 2

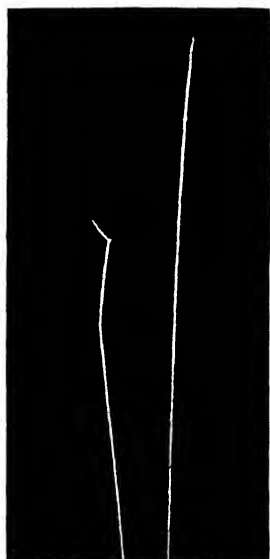


FIG. 3



FIG. 4.
PLATE I.

- FIG. 1. Alpha-ray fluorescence.
FIG. 2. Alpha-ray fluorescence: the α particles all traversed the air after its expansion.
FIG. 3. Enlargement of portion of FIG. 2.
FIG. 4. A.C.T.

I have, however, allowed the spark to traverse mercury vapour at atmospheric pressure instead of air, the brightness being thereby greatly increased.

The spark, of course, has to be suitably timed, so that the cloud trails may be illuminated after the drops composing them have grown sufficiently to scatter plenty of light but before there has been any appreciable disturbance of the air by convection currents.

Figs. 1—12 are pictures obtained by this method. It is perhaps necessary to point out that they are all photographs of clouds consisting of minute water drops condensed upon ions, as many of the clouds have a very uncloudlike appearance.

Fig. 1 is a photograph of the tracks of some alpha-particles shot out from a minute quantity of radium placed within the cloud chamber, the camera looking down through the plate-glass roof. From the atoms of radium, alpha-particles are continually being projected with velocities of many thousands of miles per second, each producing more than 100,000 ions in the course of its flight. Under ordinary conditions the trail of ions left behind by each particle is invisible; those formed by particles which have traversed the supersaturated air of the cloud chamber immediately after its expansion, however, are at once converted into visible cloud trails. These form the sharply defined spokes or rays of the picture. The more diffuse cloud rays are the tracks of particles which have traversed the air before its expansion, the ions having thus had time to wander out of the original track before losing their mobility through the condensation of water upon them. The electric field maintained in the cloud chamber fixes a limit to the age and hence to the diffuseness of the trails which are rendered visible; under the actual conditions any free ions would be driven by the electric force to the roof or floor within less than a fifth of a second after being set free. None of the ions made visible has had a free existence exceeding this limit.

It is clear that an ionising particle, while traversing or even passing near to an older trail of ions on which a cloud has already formed, will not find the vapour supersaturated to the extent necessary to cause condensation on the ions; it will therefore fail to leave a visible trail in this region. This is doubtless the reason why the sharply defined trails only appear

to begin at some distance from the source, the older trails being most closely packed in the region around the source.

By means of a suitable shutter arrangement attached to the floor of the cloud chamber, it is possible to prevent alpha-particles from traversing the moist air till after the expansion. The diffuse cloud trails are then absent from the photographs (fig. 2).

The most remarkable feature of the tracks of the alpha-particles is their general straightness. Sudden bends are to be observed, however, practically all the rays being bent within a millimetre or two of their ends. In this respect, as in others, the photographs confirm the conclusions arrived at by less direct methods.

In the next picture (fig. 3) an enlargement of two of the tracks is shown, one of them having two sudden bends. The path is otherwise straight except very near to its end. Now the alpha-particle has thousands of encounters with atoms of the gases of the air in each millimetre of its course by which ionisation is brought about, as we know from measurements made by the electrical method; and in accordance with this, the cloud particles (which are simply ions magnified by condensation of water) are so closely packed that they are not separately visible in the photograph. It is remarkable that only two encounters out of the many thousands occurring in the course of its flight should succeed in deviating the particle visibly from its course and that in these cases the deviation should be quite large.

The alpha-particle, in passing near one of the electrons of an atom, may impart to it sufficient energy to cause it to escape from the atom, whilst on account of its own enormous momentum it is not perceptibly deviated from its course. We can thus understand the general straightness of the tracks. The sudden deviations must be due to encounters of a special kind; according to Rutherford's view, such large deviations would be caused by the alpha-particle passing near the centre of the atom, where he supposes the positive charge to be concentrated.

What is perhaps the most interesting feature of the particular track I have been describing remains to be mentioned. At the second of the two bends, there is a distinct spur which one can hardly interpret otherwise than as being due to the

FIG. 5.

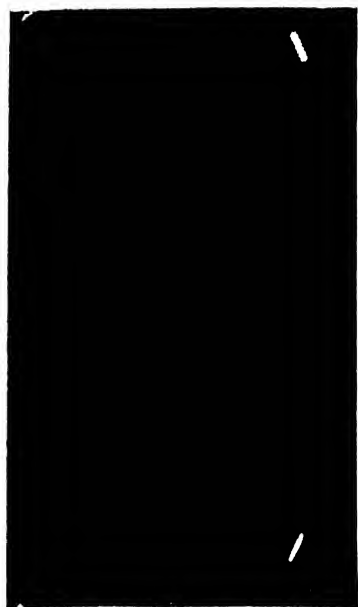


FIG. 6.

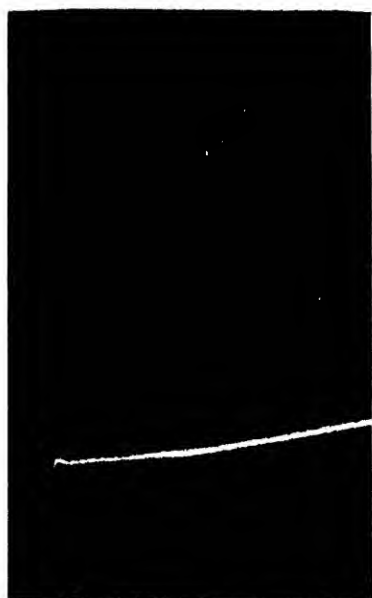


FIG. 7.



FIG. 8.



PLATE II.

FIG. 5.—Alpha-rays from radium emanation.

FIG. 6.—End portions of alpha- and beta-rays from radium.

FIG. 7.—Beta-rays from radium.

FIG. 8.—Beta- or cathode-rays excited in air by X-rays.

recoil of the system which has caused the deviation of the particle.

The next two photographs (figs. 4, 5) show the effect produced in the cloud chamber by a trace of radium emanation—the radioactive gas which is the first product of the disintegration of radium. Each cloud ray is a visible record of the conversion, by expulsion of an alpha-particle, of a single atom of the emanation into an atom of the next member of the radioactive series. Since the rays start in the gas, it is now possible to get tracks which are complete from beginning to end. The ends are distinguishable by the characteristic bend or hook. At the beginning there is an enlarged head, where, moreover, the cloud is of greater density; this represents ionisation by the recoil of the atom from which the alpha-particle has escaped.

It may be noticed there is a sudden bend in one of the rays with which there is again associated a spur-like process.

Radioactive substances emit beta-particles as well as alpha-particles. These produce comparatively few ions along their tracks, which are thus much less conspicuous when converted into visible cloud rays than those of the alpha-particles. They are, in consequence, more difficult to photograph and they have not appeared in any of the pictures shown thus far.

With suitable illumination, however, the droplets condensed on the individual ions may be photographed, provided they are not too closely packed. It is thus possible to study the path of any ionising particle, however small the number of ions produced.

On account of the enormous velocities with which they are emitted—closely approaching that of light—the beta-particles are able to travel considerable distances in the air, distances many times greater than the diameter of the cloud chamber. It is therefore impossible to obtain a picture of the whole track of a single beta-particle.

Here, on one plate (fig. 6) are shown the final portions of the tracks of an alpha- and of a beta-particle. The beta-ray shows much less intense ionisation, as indicated by the comparative densities of the clouds; and its devious path forms a great contrast to the straightness of the alpha-ray.

The beta-particle, of course, is so much more readily diverted from its course on account of its much smaller mass.

If, however, we catch the beta-particle at a sufficiently early stage of its career, we find that its immense velocity compensates

for its very small mass and its path may be sensibly straight for distances of several centimetres, in spite of the very large number of atoms which it must traverse. This is illustrated by the next picture (fig. 7) in which is shown, in addition to the end of a beta-ray, a portion of the trail left by a beta-particle while its velocity was still very high; it is noticeable that it is practically straight. Another result of the high velocity is that very few ions have been set free along its path; for the faster the particle traverses an atom the shorter is the time during which the forces can act. The individual ions are readily distinguishable in the photograph; the droplets appear mainly in pairs (each representing a positive and negative ion) but there are, in addition, here and there, closely packed groups of twenty or thirty.

In addition to the alpha- and beta-particles, radioactive bodies emit an extremely penetrating type of ionising rays—the gamma-rays—having properties similar to those of Röntgen rays. If we expose the cloud chamber to this radiation (cutting out the alpha- and beta-rays by a lead screen), we see on expansion extremely fine threads of cloud crossing the vessel in all directions. These are the tracks of beta-particles emitted mainly from the walls of the vessel under the influence of the gamma-rays. The whole of the ionisation produced by gamma-rays appears to be, as it were, secondary and due to the beta-rays.

The remaining pictures illustrate some of the properties of Röntgen rays.

In studying the nature of the process of the ionisation of air by X-rays by means of the expansion apparatus, it is convenient to use an instantaneous flash of the rays produced by sending a single Leyden jar discharge through the Crookes tube. The discharge is so timed that the rays pass through the cloud chamber immediately after the expansion of the air, so that they traverse it while it is supersaturated with water vapour. The ions produced are thus at once fixed by the condensation of water vapour upon them before any appreciable diffusion has occurred; the illuminating spark is timed to pass a fraction of a second later and so give an instantaneous photograph of the clouds condensed on the ions.

Fig. 8 is a photograph showing the effect of such a flash

of X-rays—the radiation being confined to a narrow cylindrical beam by lead screens provided with apertures. The photograph was obtained with the camera pointed horizontally through the cloud chamber in a direction at right angles to the beam of X-rays.

In the light of knowledge furnished by other methods, we may interpret the picture in the following way. Under the influence of the X-rays, an atom here and there in the path of the cylindrical beam of X-rays has emitted a corpuscle or beta-particle with velocity sufficient to enable it to traverse several millimetres or even centimetres of air, ions being set free along its path. It is the paths of these beta-particles or cathode-rays which are made visible in the photographs. The X-rays do not appear to produce any ionisation other than that effected through the agency of the beta-rays excited by them, as indeed Prof. Bragg has long maintained.

The only room for difference—apart from their mode of origin—between the beta-rays produced by the action of X-rays and those emitted spontaneously by the radioactive substances lies in their initial velocity; for there is no lack of evidence that all negatively charged corpuscles are alike, except in so far as their properties are affected by their velocity. And in fact, the tracks of the beta-particles or cathode-rays excited in air by X-rays are indistinguishable from the end portions of beta-ray tracks, such as are shown in figs. 6 and 7.

The tracks are far from straight and as the particle approaches the end of its course the deviation becomes generally more and more marked, the particle being more easily deflected the smaller its velocity.

The departure from straightness is mainly of the nature of a general curvature due to an accumulation of inappreciable deflections at successive encounters; sudden deviations through large angles, the result of single encounters of a more effective kind, also appear occasionally.

The number of ions produced per centimetre is known to increase rapidly as the velocity of the cathode-ray particle diminishes. This is shown by the increased density of the clouds towards the ends of the tracks.

Fig. 9 is an enlargement of a portion of the track of a beta-particle emitted in air exposed to X-rays. The individual ions are clearly visible and may readily be counted; the number

per centimetre amounts to about 188 pairs, when reduced to atmospheric pressure.

In taking the photograph shown in fig. 10 the X-rays were made to traverse the air before instead of after the expansion. The ions liberated along the track of each cathode-ray were thus free to move under the action of the vertical electric force maintained in the cloud chamber, the positive travelling downwards, the negative upwards. Each trail was thus divided into two portions, one consisting of negative, the other of positive ions, before being converted into visible cloudlets by expansion of the moist air; the ions of each trail have also had time to be considerably scattered by diffusion.

The representations of X-ray clouds shown thus far have all been from photographs taken with the camera pointed horizontally and so placed that a magnified image was obtained. The remaining photographs were obtained with the camera pointed vertically downwards, the conditions being such that the whole visible contents of a horizontal stratum of the cloud chamber, about 2 cm. in thickness, were photographed just as in the case of the alpha-ray pictures. Very intense illumination is required to make the cathode-ray tracks visible in a picture taken in this way and it is only recently that I have succeeded in photographing them.

A thin sheet of copper was fixed in the centre of the cloud chamber in the path of a narrow beam of X-rays, which was made to traverse the supersaturated air of the cloud chamber immediately after its expansion.

The absorption of X-rays by the copper is evident at a glance (fig. 11) from the difference of the density of the clouds condensed on the incident and transmitted beams.

In passing through the copper the X-rays produce immense numbers of cathode-rays which form dense clouds immediately in front of and behind the copper plate. The clouds are not quite in contact with the copper, the clear space next the plate being due to the air becoming warmed by contact with the copper before the passage of the rays, so that the ions fail to find the supersaturation necessary for their growth into water drops.

From the researches of Barkla and others we know that when exposed to X-rays the copper plate will emit secondary rays—the homogeneous or characteristic or fluorescent rays of copper. These will in turn cause the air to emit secondary



FIG. 9.



FIG. 10.

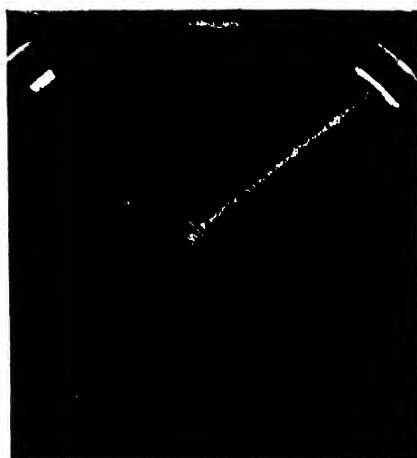


FIG. 11.



FIG. 12.

PLATE III.

FIG. 9.—Enlargement of portion of track of beta particle emitted in air exposed to X rays.

FIG. 10. Separation of positive and negative ions by an electric field in air exposed to X-rays.

FIG. 11. X-ray beam incident on thin copper plate.

FIG. 12. X-ray beam incident on thin copper plate, the less penetrating rays having been intercepted before entering the cloud chamber.

cathode or beta-rays. The visible cloud trails left by these are seen in the photograph (fig. 11). A photograph of this kind shows at once the distribution of the secondary radiation from a substance as well as the nature of the cathode-rays produced by this radiation in the surrounding gas. The cathode- or beta-rays produced in air by the copper-rays are all much alike in length (about 1 mm.); this is in striking contrast to the very varying length, ranging up to 2 or 3 cm., of those produced by the primary X-rays.

A photograph taken under similar circumstances with a silver plate in place of the copper one shows similar effects, but the cathode-rays produced in air by the silver-rays are many times as long.

Some photographs were also taken with X-rays incident upon the copper plate after their intensity had been reduced by interposing a considerable thickness of aluminium. This cuts out especially the less penetrating radiation. The individual cathode-rays which start from the copper are now readily seen (fig. 12); they were before too closely interlaced to be separately visible. The surprising feature of this photograph is the great length of some of the cathode-rays emitted by both copper and air exposed to the X-rays. Some of the tracks are about 3 cm. in length when the air is at atmospheric pressure.

HORTICULTURAL RESEARCH

III. THE ACTION OF GRASS ON TREES

By SPENCER PICKERING, F.R.S.

CONSPICUOUS among the results obtained at the Woburn Experimental Fruit Farm are those relating to the effects produced by growing grass above the roots of fruit trees. From the economic point of view the question is naturally one of considerable importance to the fruit grower but it presents a still more important aspect in its bearing on the fundamental problems of soil-fertility and the effect which one crop has on another. The mere fact that if grass be grown above the roots of fruit trees it has a deleterious effect seems to have been acknowledged previously by some growers, though it was denied, indeed, is still denied, by others. The chief reason for this divergence of opinion lies, no doubt, in the fact that the effect produced by grass varies greatly according to the nature of the soil and, in some few cases, may even be negligible: in practice also the grassing of land under fruit is generally carried out gradually, a form of treatment which materially reduces the evil effects; moreover, grassing is hardly ever practised in such a way that the grower has an opportunity of estimating by comparative trials what the effect has really been.

In the case of many soils, when the grassing is done so as to secure the maximum effect—for instance, when young trees are planted either in land already grassed or in land which is laid down to grass at once after the planting—the effect is practically always a fatal one. Fig. 1 shows two rows of standard apple trees which were strictly similar at the time of planting the one was grown in ground which was kept tilled, the other in ground which was sown with grass after the trees were planted and kept under grass. As will be seen, the result of this difference in treatment has been to arrest practically all growth. Another illustration is given in fig. 2 of similar dwarf apple trees treated in the same way; the photographs in this case were taken six years after the trees had been planted. The



Control

Control

FIG. 1.

(From *Journal of Agricultural Science*)



Fig. 1

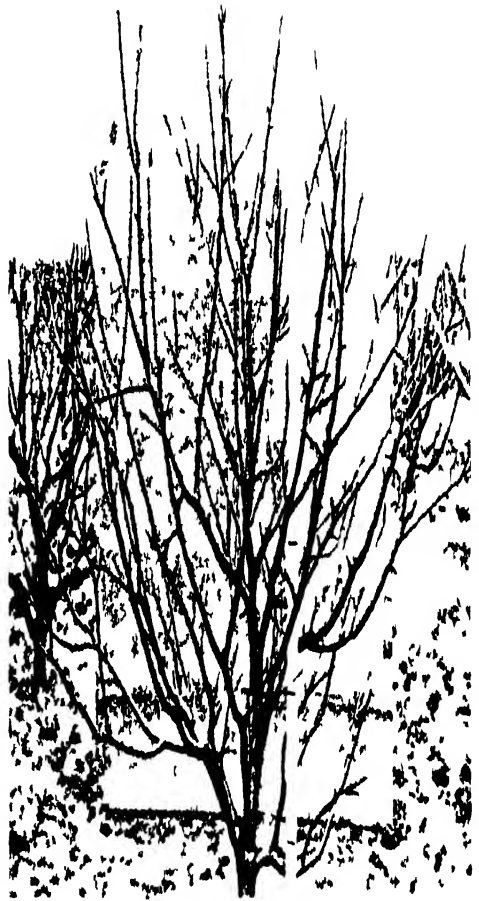


Fig. 2

Fig. 2

from the left of the tree

magnitude of the effect varies somewhat according to the variety of apple dealt with but in all cases it is very great; the effect is equally or nearly as great in the case of pears, plums or cherries and even in the case of forest trees, half a dozen kinds of which have been investigated. Certain minor modifications in the effect are noticed in some cases but it is not necessary to specify these at present.

Unless the grass be allowed to act during so long a period that the tree becomes permanently stunted, the tree will recover its vigour as soon as the ground is cleaned; in the same way, a limited recovery begins at once when any of the roots pass outside the grassed area. On the other hand, the grass-effect is noticeable when even a very small proportion of the roots are in grassed ground; for instance, when only three or four ounces of the roots of trees weighing 2 cwt. are under the grass.

It cannot be stated with certainty how far it is necessary to clear the grass away from around the roots of trees so that these may not be affected; indeed, this must evidently depend on the size and nature of the trees. In the case of freshly planted young trees, a clear space three or four feet in diameter may be advocated, though some benefit has been noticed when the cleared circle was enlarged to six feet in diameter; on the other hand, benefit has been noticed even when the grass was cleared away over a space extending only six inches away from the stems.

The grass seed usually sown in the experiments was a mixture supposed to be suitable for orchards in the particular soil in which the trials were made; but eighteen different sorts of grass have been investigated separately in experiments made with trees grown in pots and all have been found to have a similar effect, though generally the effect has been more marked in the case of the stronger growing grasses. Clover too has as great a stunting effect as grass, the only difference being that the foliage of the trees is not of the light, unhealthy colour characteristic of trees grown under grass; this difference, doubtless, is due to the extra nitrogen supplied through the agency of the nodules on the clover roots.

It was at first considered probable that the excessively deleterious action of grass was due to its having been sown around trees which had been freshly transplanted and, therefore, were not established in the soil. But this was found not to be

the case. A number of apple trees in a flourishing condition which had been in the ground four years were selected and half of them were grassed over ; the effect produced may be described as instantaneous, for the grassed trees at once ceased to produce any fresh growth and after two or three years the trees of one of the varieties dealt with were all killed. A similar experiment was subsequently made with a mixed plantation (*i.e.* one consisting of standard and dwarf apple, pear and plum trees) which had been established twelve years. The plantation was first divided into halves, so that each half contained a similar collection of trees ; on measurement the trees in these two sections were found to be of equal vigour. One section was then laid down to grass. The effect of this treatment was apparent almost at once ; and in three or four years the disturbance was so serious that, in the case of some of the varieties, the trees were actually killed ; others remained apparently unaffected for some time but are now falling considerably behind those in the tilled section.

The only case in which, in our particular soil, the action of grass seems to be modified is when the grass is allowed to establish itself gradually during the course of several years. The trees under such circumstances appear to adapt themselves to the altering conditions, though even then they do not flourish like those in tilled ground.

Many of the experiments on grassing trees have been made also in the Harpenden soil ; though the effect produced there is considerably less marked than at the Woburn Fruit Farm, it is still very conspicuous and in some cases the grassing has been fatal. In other localities, the effect of grass may be still less marked but instances of its deleterious action may be observed all over the country and in every class of soil. Only in one instance which has come under our immediate observation has there been no evident action and there seems to be no obvious reason for this failure : it is certainly not because the tree-roots have stretched down beyond the grass-roots, for both sets of roots seem to be intermingled not far below the surface.

The visible effect of grass is not confined to the arrest of growth ; it is also manifest in the altered colour of the leaves, of the bark and of the fruit. The leaves are much paler than those of healthy trees and assume their autumn tints quite a fortnight before the normal time. The bark also is pale and

unhealthy in colour, whilst the fruit is evidently lacking in green colouring matter, being either of a waxy yellow tint or showing a strong red coloration. This latter may be an advantage for market purposes and if the action of the grass could be restricted, so as merely to affect the colour of the fruit without seriously stunting the tree, it would be beneficial. This can be done in some cases by having the grass over only a small portion of the roots but the behaviour of different varieties of trees and even of different individuals of the same variety, differs too much to render such a method of culture practicable.

THE WATER SUPPLY

Naturally, the first explanation suggested was that the grass abstracted from the soil the moisture and other food materials required by the tree. Numerous experiments, however, negatived such an explanation. That grass promotes evaporation, rendering the soil drier than if the surface be kept tilled, is well known ; but it was found that this drying effect did not become appreciable until somewhat late in the year, whereas the effect of grass on the trees is manifest even in the early spring : moreover, in one season throughout which determinations were made, the drying effect of the grass was never so great that the amount of water in the soil was reduced below the optimum amount for vegetation ; and yet the trees were suffering severely. There is also the general consideration that the grass effect is manifest in wet as much as in dry seasons and that trees in tilled ground, even in the driest seasons, do not show the same symptoms as trees suffering from grass. It may further be added that in the original grassed plots at the Fruit Farm, the soil contains actually more moisture than is found in the neighbouring tilled plots : what the explanation of this difference may be is not evident ; but it is clear that the behaviour of the trees in these particular grass plots cannot be due to a diminished water supply.

Further evidence of this fact was obtained by supplying trees in grassed plots with additional moisture through pipes reaching down to their roots ; it was thus ascertained that the effect of the grass was not overcome even when the soil was kept so that there was more moisture in it than in the neighbouring tilled plots. Similar results have been obtained in other experiments

in which trees were grown in pots, the condition of moisture being so regulated that it was the same whether or no grass was present.

THE FOOD SUPPLY

Similar experiments in pots supplied the most conclusive evidence that the grass-effect is not explicable as a consequence of the lack of the recognised food material of plants any more than it is by lack of water. In some of these experiments the grass-roots were effectually prevented from coming into contact with the tree-roots by placing a layer of fine gauze about four inches below the surface and adding all the water and food from below, so that the tree obtained all that it wanted before any reached the grass. In spite of this and in spite of the supply of food being liberal, the tree suffered nearly as much from the grass as when grown in the ordinary way without gauze, the food being supplied from above.

General considerations are equally conclusive that the grass effect is not due to lack of nourishment in the soil: thus the grassed plots receive the same annual dressings of manure as do the other plots and the grass, when cut, is not removed but allowed to rot into the soil again, so that in the case of our original grassed plots nothing will have been removed from the soil during the last eighteen years other than the food material contained in the one grass crop at present on the ground together with the small amount of material removed by the feeble growth of the trees; whereas from the neighbouring tilled plot the material removed has been that contained in the annual crop of fruit and in the wood formed by the vigorously growing trees. The grassed plot must evidently be richer in food than the tilled plot: not only do analyses of the soil show that this is so but when samples of soil are taken from these two plots and trees are grown in them under similar conditions, it has been found that those in the soil from the grassed plot flourished more than twice as well as those in the soil from the tilled plot.

That the behaviour of the trees under grass is due to some form of starvation cannot be doubted—the colour of the leaf is itself proof of nitrogen starvation; but it is starvation in a land of plenty—due to the tree not being able to utilise the food which is there, not to any deficiency in the supply of that food.

It has been suggested several times that if the grass were fed off by sheep, as is the practice in the Kentish orchards, instead

of being cut, it would be found that it had no deleterious effect on the trees. This was put to the test by making several small plantations of standard apple trees in a portion of the farm which had been laid down to grass several years before and penning sheep on one of them. But during the two years throughout which this experiment lasted, the trees thus treated suffered to exactly the same extent as their neighbours in grassed land where no sheep were kept. A similar experiment is now in progress with fowls instead of sheep; the results during the first season have been equally negative, except, perhaps, that the foliage of the trees where fowls are is somewhat darker. There is one notable exception in the plantation, one of the trees showing recovered growth: but in the case of this tree the grass covering the roots has been practically eradicated by the fowls; an exception which may strictly be said to prove the rule.

OTHER SUGGESTED EXPLANATIONS

Other possible explanations have been sought in the direction of alterations produced by the grass in the physical condition of the soil, of alterations in aeration or the accumulation of carbon dioxide, of alterations in the temperature or alkalinity and also of alterations in bacterial contents. But without success.

Mechanical analysis of grassed and tilled soil failed to reveal any alteration in the distribution of the finer particles by the grass such as might give rise to the clogging of the roots by accumulating at the root level; indeed, what alteration there was has been in the opposite direction. The grassed soil also did not appear to be alkaline and when soil was made alkaline artificially, even strongly so, it did not affect the trees in the same way as the grass did; nor in the particular soil examined did it have much effect on the distribution of the finer particles.

That absence of aeration cannot be assigned as the cause seems to be fairly established by experiments described in a former article with trees having their roots enclosed by an iron drum with a layer of cement on the top; this boxing up of the tree was found to produce no effect comparable with that of grass. It was also found in this experiment that the air below the cement covering contained 50 per cent. more carbon dioxide than air drawn from below the surface of tilled ground and more than double the percentage of that in air drawn from grassed

ground, so that it is evident that the grass-effect cannot be explained by the presence of any excess of that gas in the soil. Moreover, trees grown in soil into which a current of carbon dioxide was led showed no alteration in behaviour.

The temperature of the soil under grass is on the average somewhat lower than that of tilled ground: though during the night it is slightly higher, in the daytime, under favourable circumstances, it may be as much as 10° F. lower. But the average day excess during the summer would be only about 3° and as this is less than differences observed in comparing one season with another, it is clear that it will not account for the action of the grass; added to which the grass-effect is equally apparent in the case of plants grown in pots in a greenhouse where the temperature of the soil in the various pots would be practically identical.

The possibilities of the influence of bacteria on the results have not yet been fully investigated but it is clear that the mere number of these cannot be accepted as an explanation of the grass-effect. The growth of grass is found generally to increase the number of bacteria in the soil: in certain experiments, for instance, the increase was from 2.3 to 9 million per gramme; but we may still have as great an effect of grass on the tree as occurred in this instance, when the bacterial contents is as low as 2.5 million, this being the case when the tree and grass are grown in sand instead of in earth.

THE QUESTION OF TOXICITY

A review of the whole of the facts relating to the effect of grass on trees can leave very little doubt that the action is due to some toxic effect, at any rate when this term is used in a wide sense. The tree is not deprived by the grass of the food or water necessary for its welfare; these may be present in abundance but it is incapable of utilising them: this is characteristic of a toxic action. Long before all the evidence here alluded to was obtained, such a conclusion was the one arrived at and to those who have had trees suffering from grass constantly before them, during many years, it would be difficult to arrive at any other. A toxic action, however, does not necessarily mean that the grass-roots excrete some substance which is poisonous to the tree: there is a considerable amount of debris from the roots of grass while it is growing, which on decomposition might form



No grass

Grass

Tobacco



No grass

Grass

Tobacco

Fig. 3

From *Journal of Agricultural Science*

substances poisonous to the tree-roots; or the poisonous effect might be due to an alteration in the bacterial contents of the soil.

Independently of anything coming from the grass-roots or resulting from their growth, it seemed possible that the grass might abstract something from the soil and alter the proportions of the constituents remaining so as to render the soil virtually toxic. This suggestion, however, has been negatived by some recent experiments in which the grass was grown in such a way that it was impossible for it to draw anything out of the soil in which the trees were growing. These trees were planted in pots and the grass was grown in movable trays resting on the soil in the pots; the trays were perforated to allow of drainage from them down to the trees but the holes were covered with fine gauze to prevent the grass-roots from passing through and thus there could be no passage of water upwards from the pots to the trays. Yet in spite of this entire separation of the grass from the tree, the grass-effect was still very noticeable and caused a reduction of growth amounting to some 25 per cent. These experiments have since been extended to a study of the effect of grass on other plants besides trees and in every case examined up to the present, a similar action has been observed: in the case of barley the reduction of growth amounted to 15 per cent.; in that of tomatoes to 46 per cent.; in that of mustard to 58 per cent. and in the case of tobacco to 71 per cent. Some of the results in the last two cases are shown in fig. 3. One other important point in connexion with these experiments should be mentioned, that when the grass is grown in trays as in the preceding experiments and the washings, instead of being allowed to pass immediately to the tree-roots, are left for some time exposed to the air before being used on the tree, their action, instead of being hurtful, is decidedly beneficial; apparently the toxic substance is oxidised and converted into plant-food.

The proposition which has been made to account for these facts—it cannot at present be termed more than a proposition—is that the growth of grass and probably also of other crops, gives rise, either directly or indirectly, to the formation of some substance in the soil which is toxic towards plant-growth but which, on oxidation, becomes harmless and when oxidised serves to render the soil richer, probably both in organic matter and nitrogen. While the grass is actually growing, there would be a continuous supply of this toxin which would prevent the

plants from benefiting from the increased richness of the soil; but as soon as the grass were removed, the production of toxin would cease and the previously grassed soil would be found to be more fertile than soil which had never had grass growing in it. This is in accordance with the behaviour of trees in soil from grassed and tilled land, as mentioned above; the accumulation of nitrogen in grassed land is a fact which has been known now for many years. It is probable, however, that no soil would ever be quite free from the toxic substance, if such exist, which is produced by the growth of grass.

The difficulty of examining the action of an easily oxidisable substance by means of growing plants in soil containing it is very great, because even the quickest growing plant takes a considerable time to develop; the question has been attacked, therefore, by using the germination of seeds as a means of investigation. It does not follow, of course, that a substance which is toxic towards the germination of seeds is toxic also towards plant-growth but the results indicate that this is probably so in the present case.

When soil is heated, the amount of soluble organic matter and soluble nitrogenous matter is increased; at the same time, it becomes toxic, as shown by its effect on the germination of seeds. The extent of this toxicity depends on the temperature; different seeds are affected to different extents and the results naturally are also influenced by the nature of the soil dealt with. On heating the soil to 150°C ., the soluble organic matter is sometimes increased over tenfold and the time which some seeds take to germinate in the soil increased five or sixfold. When the soil is heated to a lower temperature, the soluble matter and also the toxic effect on seeds rapidly diminishes but the latter is recognisable in soil heated to as low a temperature as 60° ; from the general form of the curve obtained on plotting the various results, it is probable that some such action exists (though it may not be measurable) in soil which has been heated only by the sun, that is, to a temperature of about 30° , so that even so-called unheated soil probably contains some of this toxic substance. This conclusion is further supported by the fact that various soils behave differently towards germinating seeds and that in nearly every case the seeds do not germinate so readily in soil as they do in pure silica moistened with water.

It was ascertained that the treatment of soils with antiseptics,

such as carbon disulphide, chloroform, ether or benzene, produced the same results as heating to a moderate temperature, the amount of soluble matter in it being increased and the soil thereby rendered slightly toxic to seeds. Such treatment was equivalent in its effect to that produced by heating the soil to about 70°; and it was impossible to attribute this to any indirect action of the antiseptic, through its modifying the bacterial growth in the soil, for it was possible to complete the whole operation of treating the soil with the antiseptic, allowing this to evaporate and obtaining an aqueous extract of the soil, within a period of from 20 to 60 minutes, during which time very little bacterial growth could have occurred; yet in this case the soluble organic matter in the soil was found to have been increased by 61 per cent. Moreover, after a soil has been treated with an antiseptic the soluble matter in it *decreases* with the lapse of time: after 18 hours the original excess of 61 per cent. was reduced to about 35 per cent. and after five weeks to 16 per cent.: so that the presence of the excess of soluble matter cannot be explained by assuming it to be the product of the growth of bacteria: it is evidently a direct product of the chemical action of the antiseptic and it is, evidently also, a very unstable product.

The conditions under which the toxic substance in heated soils is decomposed was then investigated. It was found that when the soil was kept excluded from air, even in a thoroughly wet condition, it remained unaltered, giving, after several months, the original values for the soluble matter present and for its toxic action towards germinating seeds. But if freely exposed to air and kept moistened, the amount of soluble matter rapidly decreased and at the same time it lost (in three months) its toxic properties nearly entirely. A similar but much slower change occurred when the soil was kept in a fairly dry condition.

It is clear, therefore, that the toxic substance is of an easily oxidisable nature and that it would soon be destroyed in any ordinary cultural experiments, in which free exposure to air and repeated watering have to be adopted. From the results obtained with antiseptics, it further appears that the oxidation must be very rapid at first, being considerably reduced even in a few hours, though some of the toxin may persist, as shown by the results with heated soil, after several months' exposure. In spite, however, of the toxic effect having nearly disappeared

at the end of this time, the soluble organic matter was still more than double what it was in the unheated soil and this excess of soluble matter, which is no longer toxic or is barely so, must represent the presence of so much extra plant-food; it is not surprising, therefore, to find that plants flourish much better after a time in soil which has been heated than in ordinary soil.

The occurrence of such changes in soil which has been heated renders the investigation of its behaviour towards plant-growth very difficult; it is possible that the action of the toxic substance present (if it be toxic towards plant-growth as well as towards seed-germination) may be masked, by its becoming decomposed before the plant can be grown; the only results which will follow from its presence will be an increase of growth owing to the excess of soluble organic matter left in the soil by its decomposition.

These two opposing factors are, as a matter of fact, recognisable in the results obtained when plants are grown in soil which has been heated; whether the one or the other predominate depends on the sensitiveness of the plant to the action of the toxin and on the amount of the latter present.

Fig. 4 shows tomato and tobacco plants grown in soil heated to 30° (so-called unheated soil), 60°, 80°, 100°, 125° and 150°. The presence of some toxic substance after heating to the higher temperatures is placed beyond dispute by the dwarfed condition of the plants in these cases, tobacco being evidently more sensitive to this effect than the tomato. Photographs taken at an earlier date show a much more marked effect than those given here, whilst others taken later show less effect; eventually, before growth was completed, the toxic effect had almost entirely disappeared and the beneficial effects of the products of its oxidation had so far asserted themselves that the plants, even in the most highly heated soils, had outstripped those in the unheated soil.¹ When the soil is heated to temperatures of 100° or lower, owing to the smaller quantity of toxin present, the effect persists during a still shorter period and even in the early stages we get a stronger growth than in the unheated soil.

The disappearance of the toxic effect in the most highly heated soils was further illustrated by growing a second crop of these same plants in the samples of soil used for the first crops. The results are shown in fig. 5. As will be seen, it is only in the

¹ See *Journal of Agricultural Science*, iii. 220.

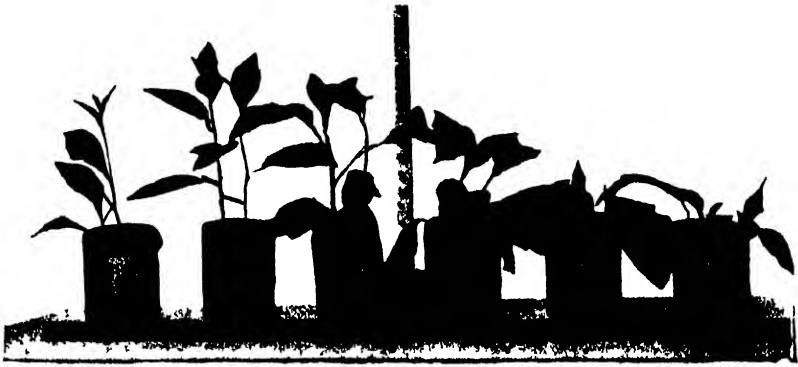


FIG. 4 — Tobacco and tomato grown in soil heated to different temperatures
First crop

(From *Journal of Agricultural Science*)

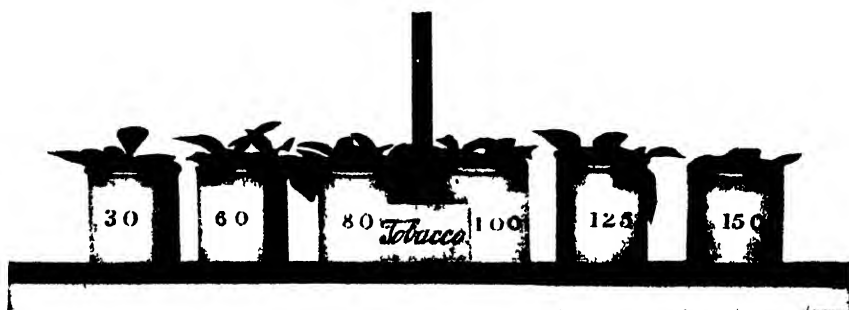


FIG. 5 Tobacco and tomatoes grown in soil heated to different temperatures.
Second crop

(From *Journal of Agricultural Science*.)

case of tobacco that any indications of toxic action are still visible and then only in the most highly heated soil.

Though the general results with all the plants examined were similar to those here described, it was noticeable that the toxic action was much less potent in the case of grasses than in that of the other plants (tomatoes, tobacco and spinach), the beneficial after-effect coming into evidence earlier and to a greater extent.

These results fully justify the conclusion that the oxidisable substance in heated soils which is toxic towards seeds, hindering their germination, is toxic also towards plant-growth.

The experiments were further extended so as to establish the identity of the action on trees with that on the other crops mentioned: in the case of trees, if the trees are grown in the ordinary way, the soil being fully exposed to air, owing to the extended time required for growth nothing is observed but the beneficial effects of the heating: but when the experiment is so modified as to limit the access of air considerably, the toxic effect is even observable. Small trees were grown in soil contained in bottles the necks of which were closed, except for two openings into which tubes plugged with cotton wool were inserted; the results of a series of experiments made in this way showed a small increase of vigour of growth, not exceeding 10 per cent., in the case of soils heated to temperatures up to 100° but a decrease, up to 35 per cent., in the case of soils heated to 125° and 150°.

The connexion between the toxic action of heated soil and the toxic action of grass on trees and other plants cannot be said to have been established yet but there are one or two facts which point to a possible identity. The soil which is toxic while the grass is growing in it does not behave normally as soon as the grass is removed; but after it has been exposed to the air, just like heated soils, it is more favourable to plant growth than ungrassed soil and contains a larger amount of soluble organic and nitrogenous matter. Another somewhat remarkable point of similarity has been noticed: soils which have been heated contain some oily or resinous substance which renders them more difficult to wet than unheated soil. This peculiarity becomes more marked as the temperature of heating is higher; different soils vary considerably in this respect. The peculiarity was so marked in one case that the soil could not be thoroughly wetted after it had been heated, even when left

in contact with water during ten days. This oiliness is noticeable, in many cases, in grassed soil, though to a less extent than in heated soils: of fourteen pairs of different samples of soil, one being taken from under grass and the other from tilled ground immediately adjoining, eight showed that the grassed soil was less readily wetted than the tilled soil.

The attempts made to discover a toxic action affecting the germination of seeds in soil from grassed land have been unsuccessful; they showed that there was a small, though undoubted, difference between the action of such soil and of soil from tilled ground but in the opposite direction, the grassed soil being the more favourable. In view of the readiness with which a small proportion of the toxin will oxidise and produce favourable results, this is not inconsistent with some toxin having been present when the samples were taken; but it cannot be used as an argument that such was the case. The fact, however, that there is some difference in action, whatever the direction may be, is more favourable to such a view than if there were no difference.

The increase of fertility produced by heating soil and by treating it with antiseptics, has recently been put to practical use in the case of soil used in greenhouses and hothouses and an explanation of the result, differing from that detailed above, has been given. According to the work of Russell and Hutchinson, when soil is heated to 50° or is treated with antiseptics, the greater number of the bacteria present and all the protozoa which feed on bacteria are killed, the result being that the surviving bacteria are able to multiply without check and soon outnumber those present in unheated soil and by this action a corresponding increase in the supply of nitrogen available for plant growth is brought about.

Without in any way controverting the evidence on which this view rests, it seems impossible to accept it as the only or even the principal explanation of the behaviour of plants in heated soil. According to it, a maximum of fertility should be observed in the case of soil heated to 50° , corresponding with the temperature at which all the protozoa are killed and the injury to the bacteria is incomplete: as the temperature of heating is raised, the fertility should decrease or at any rate should take longer to make its appearance, as a larger number of the bacteria would have been killed; and in the case of soils heated to

125° or above, in which all of them would have been killed, the soil should be much less fertile than even unheated soils or any increase in fertility which it exhibited would be of a very irregular character, depending on chance reinoculation with bacteria.

The results of growing plants in soils heated to different temperatures do not tally with these requirements. Those already alluded to are set out in fig. 6, the curve AB representing those with tobacco, tomatoes and spinach, the curve AC representing those with three grasses. This latter has been somewhat smoothed, as the values were not very regular. Neither of these curves shows a maximum at 50°: AB does show a

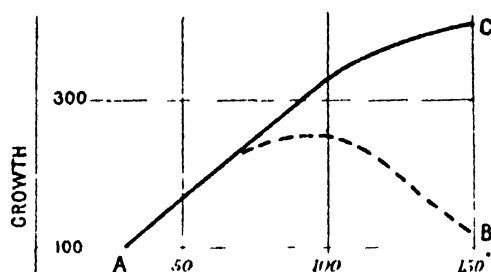


FIG. 6.

maximum but this occurs at 100° and AC shows no maximum at all. Moreover, neither curve shows any marked irregularity from 125° to 150° or any tendency to give lower values than that for the unheated soil: the results, in fact, seem to show that the circumstances conditioning them are continuous from the lowest to the highest temperature.

On the other hand these results are quite in harmony with the chemical explanation given above of the effect of heating soil—the formation of a toxic substance which becomes oxidised to form a plant-food, different plants being sensitive in different degrees to the toxic action. It is questionable, however, whether the actual quantity of plant-food thus liberated by heating to the lower temperatures, up to, say, 100°, is sufficient to explain the extra vigour of plants grown in such soil; in such cases, no doubt, the bacterial explanation of increased fertility becomes important. Both explanations are probably correct but neither alone affords a full explanation of the facts.

THE EXACT DETERMINATION OF ATOMIC WEIGHTS BY PHYSICAL METHODS

By H. F. V. LITTLE, A.R.C.S., B.Sc.

THE atomic weights of the elements are usually arrived at by measuring their combining weights as precisely as refined methods of chemical analysis or synthesis will allow and then selecting those multiples which most nearly approach the approximate atomic weights deduced with the aid of Avogadro's theorem. This may be called the chemical method.

There is, however, an alternative method of arriving at exact atomic weights, namely, to develop processes for the accurate determination of molecular weights. This may be called the physical method. The method has been developed during the last twenty years; the present article is devoted to the consideration of the results that have been obtained.

With few exceptions, the only substances of which the densities have been determined with a high degree of accuracy are those which exist as gases at ordinary temperatures and pressures; these alone will be considered in the present article. From the work of Rayleigh, Leduc, Morley, Gray and Guye and his collaborators, it may be concluded that the methods of preparing gases have been rendered so efficient and Regnault's method of determining the densities of gases has been so improved, that the densities of the commoner gases are now known with an error not exceeding 1 part in 10,000. In deducing molecular weights from these results, it is not sufficient to assume the truth of Avogadro's hypothesis in its primitive form; the fact that Boyle's Law does not *accurately* express the isothermal relationship between pressure and volume in the case of any known gas and that the coefficients of expansion of gases are not *exactly* alike is proof that even if, at some particular temperature and pressure, the relative densities of gases were accurately proportional to their molecular weights, at any other temperature and pressure this

relationship would cease to be true. As a matter of fact, the gramme-molecular volumes of gases, measured at the same temperature and pressure, are only nearly very equal. It is necessary to know the relative values of these magnitudes to within at least 1 part in 10,000 if the results of density measurements are to be utilised with advantage in the determination of molecular weights.

The problem may be stated algebraically in the following manner. Let the weight of a normal litre of a gas—*i.e.* the weight of the gas which occupies a volume of one litre at 0°C . and under a pressure of 760 mm. of mercury at sea-level in lat. 45° —be L grammes. If the gramme-molecular volume, at normal temperature and pressure, of a perfect gas be R litres, then the molecular weight M of the gas in question is not equal to RL but is given by the equation

$$M = \frac{RL}{1 + \lambda} \quad (1)$$

where λ is a small fraction to be determined. For each gas, there is a definite value of λ ; and it is necessary to determine the value of $(1 + \lambda)$ with an accuracy of 1 in 10,000. Of the various methods that have been proposed for the determination of λ , the three best known are (i) D. Berthelot's *Limiting Density Method*, (ii) P. Guye's *Reduction of Critical Constants Method* and (iii) A. Leduc's *Molecular Volume Method*.

THE LIMITING DENSITY METHOD

Boyle's Law does not accurately express the behaviour of any known gas at ordinary temperatures and under pressures of one or two atmospheres. If v_b denote the volume, under the pressure p_b , of a definite mass of a gas and v_a its volume at the same temperature as before and under another pressure p_a , we may write

$$1 - \frac{p_b v_b}{p_a v_a} = A \frac{p_b}{p} (p_b - p_a) \quad (2)$$

The coefficient $A \frac{p_b}{p_a}$ is a measure of the average error per atmosphere, over the range p_a to p_b , that is incurred by assuming the validity of Boyle's Law for the gas (it is under-

stood that pressures are expressed in atmospheres). Referring to Fig. 1 (p. 509), it will be seen that

$$A_{p_a}^{p_b} = \frac{1}{p_a v_a} \cdot \frac{p v - p_b v_b}{p_b - p_a} = \frac{1}{p_a v_a} \cdot \frac{EB}{EC}$$

Since the product $p v$ is approximately constant, it therefore follows that the coefficient $A_{p_a}^{p_b}$ is (very nearly) proportional to the slope of the chord BC of the curve ABCD joining the joints B ($p_a, p_a v_a$) and C ($p_b, p_b v_b$).

In accordance with this definition of $A_{p_a}^{p_b}$, we have

$$A_o^1 = 1 - \frac{p_1 v_1}{p_o v_o}$$

It has been assumed by Rayleigh and Berthelot (3) that, under extremely small pressures, Avogadro's hypothesis loses its approximate character; in other words, it is supposed that at a definite temperature and under a common, indefinitely small pressure, the molecular volumes of all gases are equal, an assumption which forms the basis of the method of limiting densities. The calculation of exact molecular weights by this process was given by D. Berthelot (3) in 1898 in the following manner:

Let the common molecular volume of two gases be v_o under an indefinitely small pressure p_o and at the same temperature T . When the pressure is increased to the finite value p , the molecular volumes v and v' of the gases cease to be equal. Applying equation 2 (p. 505), we have

$$v = v_o \cdot \frac{p_o}{p} \left[1 - A_{p_o}^p (p - p_o) \right]$$

$$v' = v_o \cdot \frac{p_o}{p} \left[1 - A_{p_o}'^p (p - p_o) \right]$$

The ratio of the molecular volumes is given by

$$\frac{v}{v'} = \frac{1 - A_{p_o}^p (p - p_o)}{1 - A_{p_o}'^p (p - p_o)} = \frac{1 - p \cdot A_o^p}{1 - p \cdot A_o'^p}$$

since p_o is indefinitely small.

The molecular volumes of the gases at the temperature T and under the pressure p are therefore proportional to

$$1 - p A_o^p \text{ and } 1 - p A_o'^p$$

Let the densities of the gases be L and L' respectively at the same temperature and pressure T and p . Then the molecular weights M and M' are proportional to

$$(1 - pA_0^p)L \text{ and } (1 - pA_0'^p)L'$$

A simplification may be effected in these expressions if we recall the fact that the densities are always measured at 0°C. and reduced to the values under normal pressure. By taking L and L' to represent the weights of the normal litre (p. 505) and measuring pressures in atmospheres, the previous expressions are reduced to

$$(1 - A_0^i)L \text{ and } (1 - A_0'^i)L'$$

Hence, if the weights of the normal litre of gases are $L, L', L'' \dots$, their molecular weights $M, M', M'' \dots$ are related to these magnitudes by the equations

$$\frac{M}{(1 - A_0^i)L} = \frac{M'}{(1 - A_0'^i)L'} = \frac{M''}{(1 - A_0''^i)L''} = \dots \quad (3)$$

in which $A_0^i, A_0'^i, A_0''^i \dots$ represent mean compressibility coefficients between zero and atmospheric pressures defined by equation (2) on p. 505 and measured at 0°C.

Each of the fractions expressed in (3) above is equal to R , the gramme-molecular volume of a perfect gas at normal temperature and pressure. This is seen if it be assumed for the moment that A_0^i is zero, in which case equation (3) gives

$$M/L = R \text{ or } M = LR$$

for the supposed perfect gas of molecular weight M . Hence the equalities (3) may be written

$$M = RL(1 - A_0^i), M' = RL'(1 - A_0'^i) \quad (4)$$

It follows, then, from the preceding calculations, that it is possible, from measurements of the weights of the normal litre of gases and observations of their compressibilities at 0°C. , to deduce their molecular weights and also the gramme-molecular volume of a perfect gas.

Densities.—The densities actually required for the calculation are densities referred to oxygen. Table I. gives these values at N. T. P. and also the weights of the normal litre L (see p. 505) and the critical data that will be required later on.

A few remarks upon these figures are necessary. A number

TABLE I

Gas.	L	$\frac{L}{L_{O_2}}$	Tc abs.	pe atm.
Hydrogen	0.08986	0.06288	32°	19.4
Nitrogen	1.25059	0.87515	128°	33.6
Carbon monoxide	1.25032	0.87496	133° 5'	35.5
Oxygen	1.42900	1.00000	154° 2'	50.8
Nitric oxide	1.34020	0.93786	179° 5'	71.2
Methane	0.71680	0.50161	191° 2'	54.9
Carbon dioxide	1.97678	1.38333	304° 3'	72.9
Sulphur "	2.9266	2.0480	430° 2'	77.95
Nitrous oxide	1.97791	1.38412	311° 8'	77.8
Hydrogen chloride . . .	1.63915	1.14706	324° 8'	83.6
Ammonia	0.77082	0.53941	405° 3'	109.6
Phosphine	1.5293	1.0702	324° 3'	64.5
Ethane	1.3562	0.94906	308°	45.2
Hydrogen sulphide . . .	1.5392	1.0771	373°	88.7
Methyl chloride	2.3045	1.6127	416° 3'	65.85
" oxide	2.1096	1.4763	400° 1'	53

of observers have determined directly densities with reference to oxygen. The results are set out below, together with the mean values that have been adopted here:

	H ₂	N ₂	CO	NO	N ₂ O	CO ₂
Rayleigh	—	0.87517	0.87497	—	1.38396	1.38336
Morley	0.062892	—	—	—	—	—
Guye	—	—	—	0.93789	1.38397	1.38339
Gray	—	0.87519	—	0.93782	—	—
Leduc	0.062866	0.87508	0.87495	—	1.38442	1.38324
Mean	0.062879	0.87515	0.87496	0.93786	1.38412	1.38333

Rayleigh's figure for hydrogen has been omitted, as that observer has recognised that it is too high, whilst Morley's value of the density of oxygen has been used in calculating Guye's results, since the value obtained for oxygen in Guye's laboratory was recognised to be unsatisfactory. The mean values have been converted into absolute densities by adopting Morley's value of the absolute density of oxygen. As regards the other figures in Table I., the value of L for ammonia is the mean of those due to Guye and Pintza and Perman and Davies and that for hydrogen chloride is the result obtained by Gray and Burt; the remaining values are those obtained in Guye's laboratory (14, 16, 17, 19, 23, 24).

Compressibilities.—The determination of the values of A'_0 , A''_0 . . . is a problem that at the present time cannot be regarded as solved in a perfectly satisfactory manner, except in the case of a few gases. From the definition of $A_{p_a}^{p_b}$ given in equation (2), it is clear that the value of A'_0 cannot be obtained directly from compressibility measurements but must involve an extrapolation from the lower pressure to zero pressure. The uncertainty attaching to this process will be diminished in proportion as the lowest pressure at which experimental observations are made approaches zero; but a limit is set to the extent to which p_a may be diminished by the fact that

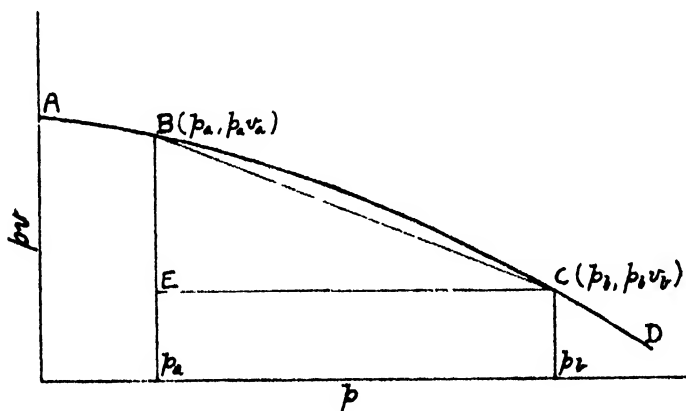


FIG 1

since the absolute error in measuring a pressure is inversely proportional to the magnitude of the latter, it eventually becomes so great that experiments at lower pressures are worthless owing to experimental errors.

The most direct and satisfactory method of determining A'_0 is to realise experimentally the 0°C . isothermal of the gas for pressures starting at one atmosphere and diminishing as far as is consistent with trustworthy results. The results should be expressed by stating the product p_v as a function of the pressure p and extrapolated to zero pressure for the value of p_v . The simplest plan is to extrapolate graphically.

Unfortunately, accurate data of this character are limited to the cases of oxygen and hydrogen chloride, for which gases the admirable experiments of Gray and Burt (20) are available.

These observers determined the values of the product p_v for these gases from $p_b = 830$ mm. to $p_a = 158$ mm. Since it will frequently be necessary in what follows to refer to diagrams in which p_v is plotted (as ordinate) against p (as abscissa), they may be conveniently called compressibility diagrams. Gray and Burt found that in the case of oxygen the compressibility graph was a straight line whilst in that of hydrogen chloride a slight but decided curvature was evident, the curve being concave to the axes of co-ordinates as indicated in figs. 1 and 2.

The results for oxygen bear out what had been previously

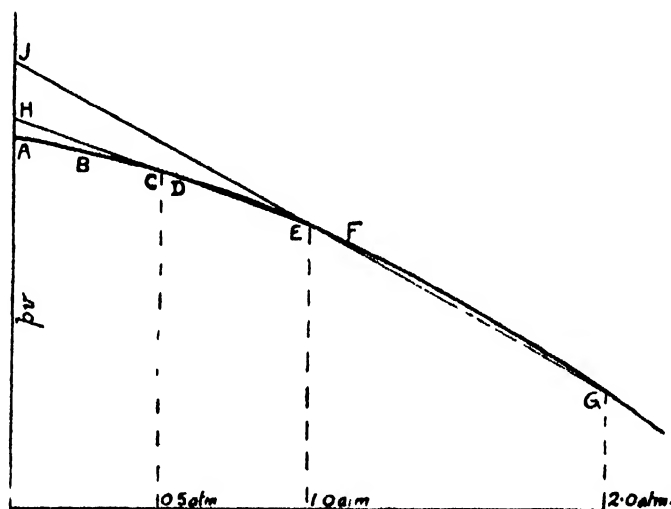


FIG 2

known since the researches of Regnault on the subject, that in the case of the difficultly liquefiable gases, p_v may, with sufficient accuracy, be regarded as a linear function of p for pressures up to three or four atmospheres. Hence it is quite simple to extrapolate to $p=0$ for these gases. Algebraically, we may say that A_0^p is a constant for values of p up to three or four; and since in the case of these gases the numerical values of this coefficient are very small and

$$A_0^1 : A_0^2 : A_0^3 :: \frac{1}{p_0 v_0} : \frac{1}{p_1 v_1} : \frac{1}{p_2 v_2}$$

we may regard either A_0^1 , or A_0^2 , as being practically identical with A_0^1 .

The various results that have been obtained for the difficultly liquefiable gases are contained in the accompanying table:

TABLE II

Observer	Values of $A \times 10^3$						
		H ₂	N ₂	CO	O ₂	NO	CH ₄
Leduc and Sacerdote (2, 21)	A_2^1	- 61	+ 38	+ 53	+ 76	+ 106	+ 175
Rayleigh (12)	$A_{0.5}^1$	53	56	81	94	—	—
Chappuis (7)	A_2^1	58	43	—	—	—	—
Jacquero and Scheuer (15)	$A_{0.5}^1$	52	—	—	97	117	—
Berthelot (13)	?	60	44	58	85	110	—
Gray and Burt (20)	A_0^1	—	—	—	96	—	—

These values refer to the compressibilities at 0° C.; it is necessary to point out that Rayleigh's measurements and also those of Leduc and Sacerdote were carried out at room temperatures and hence their values had to be reduced to those at 0° C. from theoretical considerations; also that Berthelot has merely stated his results without giving any details whatever.

In the calculations which follow, the following values will be adopted:

TABLE III

	H ₂	N ₂	CO	O ₂	NO	CH ₄
$A_0^1 \times 10^3$ at 0° C.	- 56	+ 44	+ 60	+ 96	+ 114	+ 175

To these may be added Gray and Burt's value for hydrogen chloride deduced by graphically extrapolating the compressibility curve from $p = 180$ mm. to $p = 0$:

$$A_0^1 \text{ at } 0^\circ \text{ C. for HCl} = 743 \times 10^{-5}$$

The data for other gases are not very trustworthy and will be considered later (p. 512).

Molecular Weights.—The foregoing data may now be used in calculating molecular weights, for which purpose the equation (p. 507)

$$\frac{M}{M'} = \frac{L}{L'} \cdot \frac{1 - A_0^1}{1 - A_0'^1} \quad (5)$$

is utilised. It is only necessary to substitute the numerical values of L and A_0^1 for a gas and the values of M' , L' and $A_0'^1$ for oxygen (viz. 32, 1.4290 and 96×10^{-5}), to deduce the value of M . Values of L/L' are given in Table I.

The results obtained are as follows :

TABLE IV

Gas.	H ₂	N ₂	CO	O ₂	NO	CH ₄	HCl
M . . .	2'0152	28'019	28'009	32	30'006	16'039	36'469
M (calc.)	2'016	28'020	28'000	32	30'010	16'032	36'468

To facilitate comparison, the values calculated from the International Table of Atomic Weights are given in the last line of the above table.

From the values of M just deduced (Table IV.), the following series of atomic weights is easily constructed :

TABLE V

Element	Atomic Weight (O = 16).	
	From above Molec. Weights.	From International Table.
Hydrogen . . .	1'0076	1'008
Chlorine . . .	35'461	35'46
Nitrogen . . .	14'008	14'01
Carbon . . .	12'009	12'00

These results are discussed later.

Other Compressibility Determinations.—It has been already mentioned that Gray and Burt (20) determined the values of p_v for hydrogen chloride over the range of pressure from 160 to 800 mm.; and that they found that when the values were plotted against the corresponding pressures they fell on a decided curve. The nature of this curve will be fairly evident from a consideration of the following results, deduced from their experimental data :

$$A_{0,1}^1 = 847 \times 10^{-5}; A_{0,25}^2 = 711 \times 10^{-5}; A_{0,75}^{25} = 572 \times 10^{-5}; A_1^1 = 743 \times 10^{-5}$$

Its form is evidently similar to that of the curves ABCD and ACEG in figs. 1 and 2.

The compressibility curves for other easily liquefiable gases are undoubtedly of this type, although there are few trustworthy data concerning them. These consist, for the most part, of a number of determinations of either $p_1 v_1/p_2 v_2$, or $p_1 v_1/p_2 v_1$, for various gases. In order to utilise these measurements in calculating the values of A_0^1 , it has been assumed either that the compressibility graphs are straight lines or that their curvatures may be deduced from theoretical considerations.

The values obtained on the first of these assumptions are obviously too great, as they lead to values of p_v corresponding

to the points H or J, as the case may be, instead of to the point A (fig. 2). It is necessary, therefore, to consider the theoretical views that have been applied in making the requisite extrapolations.

In the first place, it must be understood that the true form of the initial portion AB of the curve (fig. 2) is unknown. If Boyle's Law were a true statement within the limit, the curve would of course be initially horizontal, *i.e.* the tangent to the curve at A would be horizontal. On this assumption, it is easy to account for the fact that the molecular weights obtained for easily liquefiable gases by the limiting density method are usually low; the values of A_0 would have been overestimated in the extrapolation. There are no experimental data from which accurate estimates can be made of the slopes of the compressibility curves at exceedingly low pressures; but the assumption that all compressibility curves become horizontal when $p = 0$ requires that, under very small pressures, considerable changes in compressibility must occur in the case of the difficultly liquefiable gases. The validity of Boyle's Law as a "limit-law" is, however, not generally accepted; the slope of the compressibility curve at the origin is usually regarded as being qualitatively in agreement with the observed slope at atmospheric pressure. This is in accordance with van der Waals' equation and it may be remarked that most of the deductions from this equation are qualitatively correct, even though quantitative agreement may be lacking.

In calculating the values of A_0 for liquefiable gases, Berthelot (6, 13) adopts van der Waals' equation as a basis. Choosing the units so that pressures are expressed in atmospheres and the limiting value of pv when $p = 0$ is unity at 0°C. , the compressibility of a gas at 0°C. may, according to this equation, be deduced in the following manner:

$$\left(p + \frac{a}{v^2}\right)(v - b) = 1 \quad (6)$$

Neglecting the small term ab/v^2 , substituting for pb its approximate value b/v and writing e for $(a - b)$, this equation may be written

$$pv = 1 - \frac{e}{v} \quad (7)$$

i.e. the product pv is a linear function of the reciprocal of the volume.

Hence,

$$A_0^1 = 1 - p_1 v_1 / p_0 v_0 = 1 - (1 - e/v_1) = e/v_1 \\ = e/(1 - e/v_1) = e/(1 - e)$$

with a sufficient approach to accuracy ; *i.e.*

$$A_0^1 = \frac{e}{1 - e} \quad (8)$$

Berthelot also gives equations deduced from van der Waals' equation for other coefficients, viz. :

$$A_2^1 = \frac{e}{1 - 2e} ; A_1^1 = \frac{e}{1 - 2.5e} ; A_3^1 = \frac{e}{(1 - 2e)(1 - 3e)} \quad (9)$$

and from these he arrives at the following relationships :

$$A_0^1 = \frac{A_2^1}{1 + A_2^1} = \frac{A_1^1}{1 + 1.5A_1^1} = \frac{A_3^1}{1 + 4A_3^1} \quad (10)$$

by the use of which it is possible to obtain the value of A_0^1 from the results of compressibility measurements made at moderate pressures (0.5—2 atmos.).

Before applying these formulæ to the experimental data for other gases, their application to the case of hydrogen chloride may be considered. The value of A_2^1 already quoted (p. 512) leads to the following result :

$$A_0^1 \times 10^5 \text{ for HCl at } 0^\circ \text{C, from equation} = 840 \\ \text{Actual value} = 743$$

In this case, therefore, the value of A_0^1 is greatly over-estimated by formula (10). It is also interesting to utilise Gray and Burt's results to test equation (7). For this purpose, the writer has calculated the values of $1/v$ and plotted them against pv values. The points do not lie on a straight line; the graph has a curvature similar to that of the compressibility curve but not so pronounced. The high value of A_0^1 afforded by equation (10) is therefore explained. The results are interesting also from another point of view; had the measurements extended only down to 425 mm., it might very reasonably have been concluded that equation (7) was accurate, when a linear extrapolation would have given $A_0^1 = 863 \times 10^{-5}$ (about), in agreement with that deduced from equation (10) and much too high. This brings out very clearly the danger attaching to extrapolation over any considerable range of pressure; in fact, linear extrapolation of the results obtained between 158 and 265 mm. led

to the value $A_0^i = 757 \times 10^{-5}$, which is a close approximation to the actual value.

It appears, therefore, that Berthelot's method of calculating leads to results for A_0^i in excess of the true values and consequently to molecular weights that are under-estimated. The results obtained by the application of equations (10) to the available experimental data are given in the following table:

TABLE VI

Gas.	Leduc and Sacerdote (2, 21).		Chappuis (7).		Rayleigh (12).		Jacquerod and Scheuer (15).		Berthelot (13).		Berthelot (13).	
	A_0^i	A_0^i	A_0^i	A_0^i	A_0^i	A_0^i	A_0^i	A_0^i	A_0^i	A_0^i	A_0^i	A_0^i
CO ₂	681	678	694	676	666	661	—	—	688	670	676	671
N ₂ O	773	750	—	—	744	739	—	—	764	741	751	745
HCl	811	786	—	—	—	—	—	—	—	—	—	—
C ₂ H ₂	1254	1194	—	—	—	—	—	—	—	—	—	—
NH ₃	—	—	—	—	—	—	1527	1504	—	—	—	—
SO ₂	2550	2314	—	—	—	—	2386	2330	2617	2374	2407	2351
CH ₃ Cl	2739	2468	—	—	—	—	—	—	—	—	—	—

All the values given above require to be multiplied by 10^{-5} . It should be mentioned also that Rayleigh's and Leduc and Sacerdote's results were obtained at room temperatures and corrected to 0° C. by theoretical formulæ, the necessary corrections being large. Also, it should be remarked that Berthelot has merely stated his results without giving any details.

Berthelot (13) also states values of A_0^i , for the gases and deduces from his results the values of e in equation (7). From his figures, the following results have been calculated by equation (8):

	CO ₂	N ₂ O	SO ₂
$A_0^i \times 10^5$	669	743	2363

These values naturally agree with those deduced above from equation (10), since equations (10) and (8) rest on the same theoretical basis.

Jacquerod and Scheuer (15) really carried out their measurements between the pressures 800 mm. and 400 mm. and deduced values for A_0^i in a different manner. The values of $p_b v_b / p_a v_a$, when $b = 400$ mm, and $a = 200$ mm, were also determined and

the required coefficients deduced by a "parabolic extrapolation" of which no account is given. Their results were as follows :

	$10^5 \cdot A_{400}^{800}$	$10^5 \cdot A_{800}^{400}$	$10^5 \cdot A_1^1$
NH ₃ . . .	$\left\{ \begin{array}{l} 1531 \\ 1520 \end{array} \right\}$ 1526	1518	1521
SO ₂ . . .	$\left\{ \begin{array}{l} 2396 \\ 2380 \end{array} \right\}$ 2384	2360	2379

That their method of extrapolating gave too high results is highly probable since their values of A_1^1 are greater than those deduced from their measurements by Berthelot's method and given in Table VI, results which it has already been shown are probably high. Moreover, their results are not sufficiently exact to justify the extrapolation; the difference between the two values for A_{400}^{800} in the case of ammonia, is actually greater than the difference between the values they adopt for A_{400}^{800} and A_{800}^{400} . Jacqueros and Scheuer mention one source of uncertainty in the results, namely, that due to condensation of gas on the inner walls of the containing vessel, an error which was experimentally determined and allowed for in Gray and Burt's experiments on hydrogen chloride.

The uncertainty attaching to Jacqueros and Scheuer's extrapolated values may be explained by reference to fig. 2. These experimenters determined the position of the three points F, D and B only, with the object of finding E and A; and their method consisted in determining the relative positions of F and D in one experiment with a certain mass of gas and in determining the relative positions of D and B in another experiment with a different mass of gas. Hence, assuming the point F to be correctly placed, D may be in slight error with reference to it, from the first experiment; while B may be slightly in error with reference to the (already slightly incorrect) position of D, from the second experiment. There remains the error incurred by assuming a parabolic relationship between p and v . From the graphical point of view, Berthelot's method consists in determining the position of A from the known positions of only two points (E and G or E and C, as the case may be) and an assumed relationship between p and v .

Another method has also been used in arriving at the requisite compressibility values. It is obvious that at constant temperature the density of a gas which follows Boyle's Law

would be directly proportional to its pressure. In no known case, however, does this relationship hold; and the extent to which the measured densities deviate from the "theoretical" can be used in deducing the value of A_0^1 for the gas. Measurements have been made by Baume (16) of the densities of sulphur dioxide, methyl ether and methyl chloride at 0°C . and at pressures varying from 760 mm. to 311 mm. He has expressed his results by means of the equation

$$pv = 1 + m \left(1 - \frac{L_p}{L_1} \right) \quad (11)$$

in which L_p and L_1 denote the weights of a litre of gas at 0°C . and under the pressures p and 1 atmos. respectively, m being a constant. The pressure is expressed in atmospheres and at N.T.P. the product $pv = 1$. This method of extrapolation is similar in principle to that expressed in equation (7).

The following values were obtained :

SO_2	$m = 0.02381$
$(\text{CH}_3)_2\text{O}$	$m = 0.02656$
CH_3Cl	$m = 0.02215$

According to Baume (16) and also to Guye (18), we have

$$m = A_0^1$$

This conclusion, however, is erroneous and is due to the fact that Baume, in his paper, defines $A_{p_1}^{p_2}$, in two different ways which are not equivalent. As a matter of fact, we have from equation (11),

$$p_1 v_1 = 1 \text{ and } p_0 v_0 = 1 + m,$$

and since, from equation (2),

$$A_0^1 = 1 - p_1 v_1 / p_0 v_0,$$

it follows that

$$A_0^1 = 1 - (1 + m)^{-1} = \frac{m}{1 + m} \quad (12)$$

The above values accordingly lead to the coefficients :

SO_2	.	.	.	$A_0^1 = 0.02325$
$(\text{CH}_3)_2\text{O}$.	.	.	$= 0.02587$
CH_3Cl	.	.	.	$= 0.02167$

which are much lower than those adopted by the Geneva experimenters; further, the agreement between the values for sulphur dioxide obtained by this method and by that used by Jacquerod and Scheuer (p. 516) vanishes. It will be noted, however, that the figure for sulphur dioxide, obtained by

correcting Baume's calculation, agrees well with that obtained by applying Berthelot's method to Jacquerod and Scheuer's compressibility measurements.

Excluding the values of A_0^1 for ammonia and sulphur dioxide given by Jacquerod and Scheuer, the preceding results may be summed up as follows :

TABLE VII

Gas.	$A_0^1 \times 10^5$	M	Accurate Molecular Weight.
CO ₂	661 to 678	44'017 to 44'009	44 000
N ₂ O	739 to 757	44'007 to 43'999	44'020
C ₂ H ₄	1194	30'037	30'048
NH ₃	1504	17'018	17'034
SO ₂	2314 to 2374	64'082 to 64'043	64'070
(CH ₃) ₂ O	2587	46'064	46'048
CH ₃ Cl	2167 to 2468	50'537 to 50'381	50 484

The molecular weights corresponding to the extreme values of A_0^1 given in column 2 are given in column 3, whilst column 4 contains the molecular weights calculated from the International Atomic Weights.

In view of the uncertainty attaching to these values, a detailed discussion is unnecessary. It is, however, obvious that the above values of M cannot afford accurate atomic weight values. Further measurements of compressibilities at 0°C. sufficiently comprehensive to reduce the uncertainty attaching to the extrapolation to the smallest possible dimensions are required; Gray and Burt's method, which determines A_0^1 from first principles, as it were, is undoubtedly the best of those hitherto used.

REDUCTION OF CRITICAL CONSTANTS METHOD

In this method of determining molecular weights, published by Guye (9) in 1904, the requisite data, in addition to the normal densities of gases, are their critical temperatures and pressures; the determination of A_0^1 from compressibility measurements, a difficult task as the preceding discussion has shown, is unnecessary.

The fundamental formula is derived from van der Waals' equation, which Guye applies in a form slightly different from that used by Berthelot. Measuring pressures in atmospheres

and choosing as the unit of volume the volume occupied by the gas at N. T. P., van der Waals' equation becomes

$$\left(p + \frac{a}{v^2}\right)(v - b) = (1 + a)(1 - b)(1 + at) \quad (13)$$

where $a = 1/273$ and t = temperature in degrees Centigrade. This is the equation used by Guye. Assuming the validity of the fundamental assumption of the method of limiting densities and also assuming that equation (13) represents the behaviour of a gas between 0 and 1 atmos., Guye's fundamental formula follows readily from equation (4) on p. 507, viz. :

$$M = RL(1 - A_0') \quad (4)$$

For, since

$$A_0' = 1 - \frac{p_0 v_0}{p_0 v_0} = 1 - \frac{1}{p_0 v_0}$$

at 0° C. with the preceding choice of units, the equation (4) may be written

$$M = \frac{RL}{p_0 v_0} \quad (14)$$

Also, at 0° C. equation (13) may be written

$$pv = (1 + a)(1 - b) + pb - \frac{a}{v} + \frac{ab}{v^2}$$

and since, when $p = 0$, $v = \infty$, we have

$$p_0 v_0 = (1 + a)(1 - b).$$

Hence equation (14) becomes

$$M = \frac{RL}{(1 + a)(1 - b)} \quad (15)$$

which is Guye's formula.

Guye arrived at his formula in the following manner. It has been shown by van der Waals (4) and independently by Guye and Friderich (5) that the acceptance of van der Waals' equation leads to the following result:

At normal temperature and pressure, the relative volumes of different gases that contain equal numbers of molecules are proportional to

$$\frac{1}{(1 + a)(1 - b)}, \frac{1}{(1 + a')(1 - b')}, \frac{1}{(1 + a'')(1 - b'')} \dots$$

the accents referring to different gases, the units being chosen as previously described.

As the molecular weights ($M, M', M'' \dots$) are proportional

to the products of these expressions into the respective values $L, L', L'' \dots$, it follows that

$$\frac{\frac{M}{L}}{(1+a)(1-b)} = \frac{\frac{M'}{L'}}{(1+a')(1-b')} = \frac{\frac{M''}{L''}}{(1+a'')(1-b'')} = \dots = R \quad (16)$$

corresponding to equations (3) on p. 507; it is obvious that each fraction is equal to R (cf. p. 507). Hence,

$$M = \frac{RL}{(1+a)(1-b)}, M' = \frac{RL'}{(1+a')(1-b')} \dots \quad (15)$$

an expression for M identical with that previously obtained.

Guye adopts the value $R=22.412$. The calculation of the values of a and b depends upon a knowledge of T_c and p_c , the critical temperature (absolute) and pressure of the gas. The following equalities, connecting a, b, T_c and p_c , can be deduced from theoretical considerations in connexion with van der Waals' equation :

$$p_c = \frac{a}{27b^2}, T_c = \frac{8a}{27bR} = \frac{8 \times 273a}{27b(1+a)(1-b)} \quad (16)$$

By solving these equations, a and b may be expressed in terms of T_c and p_c , magnitudes which can be experimentally determined. The calculation involves the solution of a cubic equation and the numerical values of a and b are best obtained by the ingenious method given by Haentschel (11). Values of b calculated in this manner agree very well with those deduced from the equation

$$b = 0.0004496 \frac{T_c}{p_c} + 0.000001835 \left(\frac{T_c}{p_c} \right)^2$$

given by Guye and Friderich; this equation therefore affords a simple means of approximating to b for any gas. All values of a and b quoted later, however, have been calculated by solving the necessary cubics.

Whilst the fundamental equation

$$M = \frac{22.412 L}{(1+a)(1-b)} \quad (17)$$

has the theoretical significance that attaches to a deduction from van der Waals' equation, it is, like van der Waals' equation itself, only approximately correct and therefore Guye has modified it. The modified equations, however, can only be regarded as empirical, as will be seen subsequently.

Let us consider how Guye modifies his equation in order to deduce the molecular weights of *readily liquefiable* gases. In common with van der Waals, van Laar and others, he supposes that the original van der Waals' equation can be made to represent accurately the behaviour of a gas if the values of a and b are assumed to vary with temperature and pressure. Therefore, he regards the values of a and b calculated from critical data as being valid at the critical temperature and proposes two empirical equations from which to determine $a_{p,T}$ and $b_{p,T}$, the values of a and b at p , T . These equations are :

$$a_{p,T} = a \left(\frac{T_c}{T} \right)^{\frac{1}{2}}, \quad b_{p,T} = b \left(1 + \frac{T_c - T}{T_c} \right) \left(1 - \beta \frac{p_c}{p} \right) \quad (18)$$

β being a constant common to all the gases. The values a_0 and b_0 assumed by a and b at N. T. P. are therefore

$$a_0 = a \left(\frac{T_c}{273} \right)^{\frac{1}{2}}, \quad b_0 = b \left(1 + \frac{T_c - 273}{T_c} \right) \left(1 - \beta p_c \right) \quad (19)$$

Using these values, Guye calculates the molecular weights from the equation

$$M = \frac{22.412 L}{(1 + a_0)(1 - b_0)} \quad (20)$$

The empirical character of equations (18) is sufficient to nullify the theoretical value of Guye's method. Moreover, the demonstration that, at N. T. P., the relative volumes of different gases that contain equal numbers of molecules are proportional to expressions of the type $1/(1 + a)(1 - b)$ is based upon the assumption that b is independent of the pressure; therefore as soon as equation (18) for $b_{p,T}$ is accepted, the proof of formula (15) becomes invalid.

The expression for b_0 is easily seen to be, at the best, of limited application. In the cases of hydrogen, nitrogen and carbon monoxide it gives negative values, an absurd result when the theoretical meaning of b_0 is considered. Moreover, according to formula (18) $b_{p,T}$ approaches $\pm \infty$ as the pressure approaches zero.

As has been already mentioned, Guye only applies the preceding calculations to the readily liquefiable gases. In order to determine the numerical value of β , he adopts carbon dioxide as the standard gas and takes as the atomic weight of carbon the value 12.002 deduced from gravimetric measurements of the ratios $C : CO_2$ and $CO : CO_2$. Hence $M = 44.002$ and

accepting the values for L , T_c and p_c given in Table I., it follows that

$$\beta = 0.003223$$

The molecular weights calculated from equations (16), (19) and (20) are as follows (for values of L , T_c and p_c refer to Table I.):

TABLE VIII

	$a \times 10^5$	$b \times 10^5$	$a_0 \times 10^5$	$b_0 \times 10^5$	M
Carbon dioxide	721	191	847	161	44.002
Nitrous oxide	719	185	878	156	44.012
Ammonia	859	170	1554	146	17.036
Phosphine	940	233	1217	214	33.935
Ethane	1209	314	1449	299	30.051
Hydrogen chloride . . .	722	179	937	152	36.451
Hydrogen sulphide . . .	900	194	1438	240	34.085
Sulphur dioxide	—	—	2837	267	63.954
Methyl chloride	—	—	2872	310	50.363
Methyl oxide	—	—	3111	382	46.030

In the case of the *difficultly liquefiable* gases, Guye uses a simpler method of calculation. The molecular weights given by equation (15) are found to be too low by amounts proportional to the critical temperatures (absolute) of the gases; hence instead of (15) Guye writes

$$\frac{M}{L}(1+a)(1-b) = R + mT_c \quad (21)$$

where m is a constant for the gases and a and b are deduced by means of equation (16). To determine the actual value of m , the numerical values of M , L , a , b and T_c for oxygen are substituted in the equation (R equals 22.412 as before), the result being that

$$m = 0.0000623.$$

The following table contains the results obtained by the application of equations (16) and (21) to the data for the difficultly liquefiable gases (values of L , T_c and p_c are given in Table I.):

TABLE IX

	$a \times 10^5$	$b \times 10^5$	M
Hydrogen	28.8	73.7	2.0150
Nitrogen	275	174	28.013
Carbon monoxide . . .	284	172	28.003
Oxygen	266	139	32
Nitric oxide	257	115	30.009
Methane	379	169	16.034

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From the values of M given in the last two tables, the following atomic weights are readily obtained :

TABLE X

Hydrogen.	Carbon.
1'0075 from H_2 .	12'003 from CO .
Nitrogen.	12'004 " CH_4 .
14'007 from N_2 .	12'002 " CO_2 .
14'009 " NO .	12'003 " C_2H_6 .
14'012 " N_2O .	12'003 = mean.
14'013 " NH_3 .	
14'010 = mean.	Phosphorus.
Chlorine.	30'912 from PH_3 .
35'436 from HCl .	Sulphur.
	22'070 from H_2S .
	31'954 " SO_2 .

These results are discussed later.

It is convenient here to refer to another method of calculating molecular weights, due to Berthelot, which also requires a knowledge of L , T_c and p_c . In the course of an elaborate discussion of the compressibilities of gases between 0 and 3 atmospheres, based largely upon the experimental results obtained by Chappuis, Berthelot (6) was led to propose a characteristic equation for gases which is of the same form as that given by van der Waals but in which a and b are not constants. The values at N.T.P. according to Berthelot, are

$$a = 10^{-8} \times 2'071 \times T_c^2/p_c, \quad b = 10^{-4} \times 2'575 \times T_c/p_c$$

the units of pressure and volume being those already explained (p. 513) in connexion with Berthelot's other method. Denoting $(a - b)$ by e , we have as before (p. 514)

$$A_0^1 = e/1 - e$$

and the calculation of molecular weights is made by the "limiting-density," formula (5) on p. 511. Berthelot calls this the "indirect" method of limiting densities.

The following values of $10^5 \cdot A_0^1$ are obtained from the critical data given in Table I.:

TABLE XI

Gas.	$10^5 \cdot A_0^1$	Gas.	$10^5 \cdot A_0^1$	Gas.	$10^5 \cdot A_0^1$	Gas.	$10^5 \cdot A_0^1$
H_2	- 39	NO	103	CH_4	1177	SH_2	1116
N_2	+ 31	CH_4	174	HCl	755	SO_2	2013
CO	42	CO_2	698	NH_3	1177	CH_3C	2152
O_2	71	N_2O	709	PH_3	974	$(CH)_2O$	2363

In the case of the first ten gases mentioned in the preceding table, the molecular weights calculated from the values of A_c^1 given agree well with those derived from the International Table of Atomic Weights but considerable discrepancies occur in the case of the remaining gases.

Another equation deduced by Berthelot (6) should also be mentioned, as it has given rise to some misunderstanding. The equation is

$$\frac{1}{\pi\nu} \cdot \frac{d(\pi\nu)}{d\pi} = \frac{9}{128\theta} \left(1 - \frac{6}{\theta^2}\right)$$

in which π , ν and θ denote the "reduced" pressure, volume and abs. temperature of a gas (*i.e.* these magnitudes expressed as fractions of the critical values). This equation is only valid *when π is indefinitely small*; in other words $\frac{d(\pi\nu)}{d\pi}$ only gives the slope of the compressibility curve at A (figs. 1 and 2). This point has escaped the notice of Guye and his collaborators, who quote the above equation in its equivalent form (at 0° C.)

$$A = 0.0002575 \frac{T_c}{p_c} \left(\frac{6T_c^3}{273^3} - 1 \right)$$

and refer to it as Berthelot's indirect formula *for A_c^1* . Such is, of course, not the case; the formula refers only to the limiting value of $A_{p_a}^{p_b}$ when both p_a and p_b approach zero, a value considerably smaller than A_c^1 .

It was by utilising this formula that Rayleigh (12) reduced his compressibility measurements to the values at 0° C.

THE MOLECULAR VOLUME METHOD

The method of molecular volumes was chronologically the first of the methods described in this article (2). It will be seen that while in principle it may be identified with the method of limiting densities, yet with respect to the experimental data necessary, *viz.* densities and critical constants, it resembles the method of critical constants. Unlike the latter, however, it is not a deduction from van der Waals' equation empirically modified but rests on the broader basis of the Theorem of Corresponding States.

The account here given is, in substance, that contained in Leduc's latest memoir on the subject (21). The pressure, molecular volume and absolute temperature of a perfect gas.

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i.e. one for which the laws of Boyle and Gay Lussac hold exactly, are connected by the relationship

$$pV = KT$$

where K is a constant which, by Avogadro's theorem, has the same value for all perfect gases. But no known gas is perfect and if, at the temperature T and pressure p , the molecular volume V' of a gas be expressed by the equation

$$pV' = K'T \tag{22}$$

the value of K' is not identical with K . By division,

$$K'/K = V'/V$$

The ratio V'/V , *i.e.* the ratio of the molecular volume of a gas to the molecular volume of a perfect gas at the same temperature and pressure, will be called ϕ . Since, then, ϕ is equal to K'/K , equation (22) may be written

$$pV' = KT\phi$$

or, if M be the molecular weight of the gas and v its specific volume,

$$Mpv = KT\phi \tag{23}$$

which is Leduc's method of writing the equation. It must be noted that, in this equation, ϕ is a *variable* quantity.

For the particular case of oxygen, we may write

$$32 pv_{O_2} = KT\phi_{O_2}$$

whence the molecular weight of a gas is seen to be given by the equation

$$\frac{M}{32} = \frac{\phi v_{O_2}}{\phi_{O_2} v}$$

This in turn may be written

$$\frac{M}{32} = \frac{\phi}{\phi_{O_2}} \cdot \frac{d}{d_{O_2}} \tag{24}$$

In this equation d and d_{O_2} denote the densities of the gas and oxygen at the temperature T and pressure p , whilst ϕ and ϕ_{O_2} refer to the same temperature and pressure.

It remains to indicate the manner in which Leduc arrives at the values of ϕ and ϕ_{O_2} . Referring back to equation (23) and

indicating by zero suffixes the values of the variables at temperature T and at zero pressure,

$$Mp_0v_0 = KT\phi_0 \quad (25)$$

Hence, from (23) and (25),

$$\frac{pv}{p_0v_0} = \frac{\phi}{\phi_0}$$

Leduc assumes that ϕ_0 is sensibly equal to 1, *i.e.* at a common temperature and under a common, indefinitely small pressure, all gases have the same molecular volume. This is, of course, the fundamental assumption of D. Berthelot's method. Hence,

$$\phi = pv/p_0v_0$$

Further, Leduc assumes that over the range of a few atmospheres pressure, the compressibility of a gas may be represented by the equation

$$E = 1 - \frac{pv}{p_0v_0} = mp + np^2,$$

where m and n are small constants to be determined for each gas.¹ Hence,

$$\begin{aligned} \phi &= 1 - mp - np^2, \\ \text{i.e. } \phi &= 1 - mp_c \left(\frac{p}{p_c}\right) - np_c^2 \left(\frac{p}{p_c}\right)^2 \end{aligned} \quad (26)$$

Leduc measures p in cms. of mercury and p_c in atmospheres and denotes p/p_c by e , so that

$$\phi = 1 - mp_c \cdot e - np_c^2 \cdot e^2 \quad (27)$$

The values of m and n are arrived at by an application of the theorem of corresponding states. *At the same "reduced" temperature and "reduced" pressure, the molecular volumes of gases are assumed to be equal.* The "reduced" temperature and "reduced" pressure of a substance are T/T_c and p/p_c respectively, *i.e.* the temperature and pressure expressed as fractions of the critical values. Accordingly, for the same value of the "reduced" pressure (or e), different gases give the same values for ϕ when at the same "reduced" temperature. Hence, in equation (27), the coefficients mp_c and np_c^2 must be functions of the "reduced" temperature only. Leduc calls the reciprocal

¹ If for ϕ and ϕ_0 in equation (24) the values $1-E$ and $1-E_0$ are inserted, it will be immediately seen that the equation is identical with that derived from the method of limiting densities.

or the reduced temperature χ and deduces two equations to represent the values of mp_c and np_c^2 as functions of χ , within the limits of experimental error. These equations are

$$10^4 \cdot mp_c = 18.85\chi (2\chi^3 - \sqrt{2}\chi + 2\sqrt{2}\chi - 1) \quad (28)$$

and

$$10^4 \cdot np_c^2 = 3.5\chi^3(\chi - 1) \quad (29)$$

which represent the results of Leduc's experiments on the compressibilities of gases with great accuracy.¹

The molecular volume method is applied in the following manner. Given the density d of a gas at temperature T and pressure p and given its critical temperature T_c and critical pressure p_c : required its molecular weight. Since p_c is given and χ , which equals T_c/T , is also known, equations (28) and (29) enable m and n to be calculated. Equation (27) then gives the value of ϕ , as e , which equals p/p_c , is also known. It is then necessary to be able to calculate ϕ_{O_2} , the value for oxygen at the same temperature T and pressure p and the required molecular weight follows from equation (24). In practice, T and p are 273° and 1 atmos. respectively.

Leduc found that the theorem of corresponding states could not be applied to certain gases, *i.e.* that equations (28) and (29), which give the correct values of m and n for a large number of gases, do not give correct results in these particular cases. These exceptional gases are ammonia, phosphine, hydrogen sulphide and methylic ether. On the other hand, equations (28) and (29) derived from data relating to substances gaseous at the ordinary temperature and pressure may be successfully applied to the calculation of A_2 , for toluene vapour at 129.6° C., the result being in excellent agreement with that obtained from the experimental data of Ramsay and Steele (8).

Since equations (28) and (29) rest largely upon the compressibility data of Leduc and Sacerdote and upon critical constants which often differ a little from those hitherto employed in this article, a detailed statement of the numerical results obtained by this method is unnecessary, as the values would not be directly comparable with those already deduced by other methods. It is obvious that the results obtained by this

¹ Leduc deduced an expression for A_2 in terms of m and n and then sought equations for m and n which would enable him to reproduce his experimental values of A_2 .

method cannot differ sensibly from those obtained by the limiting density method, except in so far as errors are incurred in effecting extrapolations.

Leduc has arrived at the following atomic weights: $H = 1.0075$, $N = 14.006$, $C = 12.005$, $Cl = 35.45$ (probably low).

COMPARISON OF ATOMIC WEIGHTS DERIVED (i) BY CHEMICAL
ANALYSIS AND (ii) BY PHYSICAL METHODS

The atomic weights deduced from the most trustworthy data by the methods described in this article are as here tabulated :

TABLE XII

	Density Limits	Critical Constants.	Molecular Volumes.
Hydrogen	1.0076	1.0075	1.0075
Nitrogen	14.008	14.010	14.006
Carbon	12.009	12.003	12.005
Chlorine	35.461	35.436	35.45

The values for sulphur are unsatisfactory. The value for phosphorus deduced by Guye's method is undoubtedly too low and the same remark applies to Guye's value for chlorine. These low values may possibly arise from a slight "association" of hydrogen chloride and phosphine, the degree of association varying between N.T.P. and the critical temperature and pressure (22). Leduc (21) criticises his value for chlorine as being, if anything, too low.

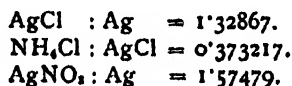
The atomic weight of carbon obtained by these methods approximates closely to the result obtained from the best gravimetric work. The rather high value obtained by Berthelot's method suggests that the compressibilities of carbon monoxide and methane need revision, a conclusion that may also be drawn from an inspection of the compressibility measurements given on p. 511.

The atomic weight of hydrogen quoted above is in agreement with the results of the best gravimetric work on the composition of water but is distinctly an indication of the superior accuracy of Morley's value (25) 1.0076 over that obtained subsequently by Noyes (27), viz. 1.0078. Other considerations point to the same conclusion.

Special interest attaches to the atomic weight of nitrogen, to which the physical methods assign a value slightly lower than

14'01. The value 14'003 was obtained by the physical method as early as 1895 by Rayleigh and Ramsay (1) but the value 14'04, derived mainly from the work of Stas, was published in the International Table as late as 1906. Meanwhile, the low figure was confirmed by Leduc (2), Berthelot (3), Guye and his collaborators (10) and Gray, all using the physical method. The first chemical work to yield the low value 14'01 was the analysis of nitrous oxide by Guye and Bogdan (40) in 1904 and after this result was confirmed by Gray's analysis of nitric oxide (39), the value $N = 14'01$ was adopted in the International Table.

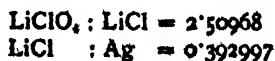
Ladenburg's discovery (26) in 1902 of an error in Stas's value for the atomic weight of iodine was followed some years later by the discovery, due to Richards and Wells (29), that Stas's value for chlorine was in serious error. Since then, the values of the fundamental atomic weights have been subjected to a most careful revision, in the course of which the value 14'01 for nitrogen has received further confirmation. The three following ratios have been determined with the utmost care by Richards and others (29, 30, 31):



From these results, assuming with Morley that $H = 1'0076$,¹ it is easy to deduce that $N = 14'009$, $\text{Cl} = 35'457$, $\text{Ag} = 107'88$. The value here given for silver was proposed by Guye (10) in 1905, as a necessary consequence of adopting the value 14'01 for nitrogen and has now been substituted for the old value 107'93, due to Stas.

To the analytical evidence in favour of $N = 14'01$ already quoted, it is necessary to add the analysis of nitrogen peroxide by Guye and Drouguine (36), from which the value 14'009 was deduced, also the synthesis of the peroxide from nitric oxide and oxygen, from which Wourtsel (38) deduced the value 14'007.

Turning to the atomic weight of chlorine, the value 35'457 derived above received the following confirmation. Firstly, the ratios



established by Richards and Willard (33), lead to $\text{Cl} = 35'454$

The assumption that $H = 1'0078$ leads to almost identical results.

and $\text{Ag} = 107.871$. Secondly, the work of Richards and Staehler (32) affords the ratio

$$\text{K} : \text{Cl} = 1.102641$$

which, combined with Staehler and Meyer's ratio (37)

$$\text{KClO}_3 : \text{KCl} = 1.643819$$

leads to the value $\text{Cl} = 35.458$.

The mean value $\text{Cl} = 35.456$ derived from these gravimetric results is in agreement with the value 35.461 deduced by Gray and Burt, using the method of limiting densities; and if Morley's values for the density and atomic weight of hydrogen are admitted, further confirmation is supplied by Edgar's syntheses of hydrogen chloride (34), which give $\text{Cl} = 35.461$ and Gray and Burt's volumetric analyses of hydrogen chloride (20), which give $\text{Cl} = 35.459$. It should be mentioned, however, that Noyes and Weber's syntheses of hydrogen chloride (28) supply the distinctly low value 35.452 , whilst the analyses of nitrosyl chloride by Guye and Fluss (35) furnish a decidedly high result, viz. 35.466 .

In conclusion, it would appear that the physical methods have led to the deduction of several fundamental atomic weights, which, in point of accuracy, compare favourably with the values derived from the best chemical work that has been accomplished. It is to be hoped that subsequent research will add to their number: as has been already indicated, a considerable amount of work still remains to be done on the subject of gaseous compressibilities.

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THE LOGIC OF DARWINISM

By ARCHER WILDE

By common consent, the great discovery of Darwin and Wallace has long been considered to be as fully and finally established as one of the most important of natural laws; their names are enrolled among the immortals and their work forms the base upon which all must take their stand who would peer yet further into the secrets of life. Yet Darwinism still seems new and its bearings even on strictly biological problems are far from being fully worked out. It has been stated recently that "Biology to-day teems with mutually incongruous opinions." The science has hardly emerged from the state of ferment into which it was thrown by a discovery which utterly subverted the old order while necessarily supplying, at first, only the framework of the new. There is therefore the less reason for surprise if, as I shall attempt to show, the logical proof upon which the theory of Natural Selection rests be not justly estimated by the educated world at large. Some may perhaps ask—as long as the theory is fully accepted, what does it matter upon what grounds it may be based? but I feel sure that more will agree with the view that the great importance of the subject and its intimate bearing upon social and political questions render superfluous any apology for an endeavour to secure a fresh survey of the ground on which Darwin built, if any reasonable cause can be shown for it.

I have long held the opinion that the strength of Darwin's argument has been seriously under-estimated in this—that the theory is regarded as still awaiting the final proof afforded by experiment. Whether or no this may be partly a lingering effect of his great and possibly even excessive modesty is an interesting question which I cannot now touch; the fact remains that, even among the most convinced supporters of the theory of Natural Selection, it is common to find writers who state or imply that the theory is susceptible, in this way, of a higher kind of proof than it has yet received. For instance,

the able author of a little book on "Organic Evolution," written a few years since for the instruction of the public, makes the admission, in replying to objectors, that "we have not seen natural selection at work"; and he propounds the opinion that, for final proof, we have to await the result of certain observations then being made by Prof. Weldon on crabs in Plymouth Sound, which he regards or regarded, as far as they had proceeded at the time of writing, as "very nearly tantamount to experimental proof of the theory of natural selection." Other quotations which I shall subsequently make show that this opinion is still commonly accepted. The point which I shall here endeavour to establish is that this attitude of mind is mistaken: that the Darwinian theory has long since received the highest proof possible—the proof of experiment—and is incapable of further verification, except in the sense in which the theory of gravitation is still being verified by the continual accumulation of additional instances in which the phenomena of nature are found to conform with the law.

Experiment differs from ordinary observation only in this, that the phenomena observed are as far as possible kept under control and isolated from the operation of the surrounding forces of nature. Thus, instead of observing the effects produced by a particular acid upon a particular metal as these occur in nature, which would be difficult if not impossible, we isolate them both as far as possible and then bring them into contact and observe their interaction. So, for instance, it is found that the interaction of copper and sulphuric acid gives rise to the beautiful blue vitriol of commerce. Now the domestication of plants and animals, which began ages ago; and the improvement of breeds, which advanced gradually, in the course of thousands of years, through unconscious to conscious selection, until in recent times, especially since Darwin and Wallace published their joint discovery, the deliberate improvement of stock by selection of the most useful or most fancied strains has become the common practice of every breeder: what are these but the isolation and control of the phenomena of reproduction in the organic world, attended as they are by careful observation and usually by the maintenance, in modern times, of a complete record of results? What then does the process amount to but one long series comprising an infinity of individual experiments in proof of the Darwinian theory? Not

of course that the attempts were purposely made as experiments in proof of any theory whatever. The guiding purpose has always been man's own advantage: the fancier's love of his hobby or the breeder's profit. But is it of the essence of an experiment that it should be purposely made as such to prove a theory? I think not. All that is really essential is a sufficient control of the phenomena and a sufficient observation and the record of the sequence of events, all of which we undoubtedly have. In a word, it is possible to prove theories by experiment without knowing that we are doing so; this is what has been done. Breeding is the experimental production of variety by the selection of variations.

To see the force of this contention, it is only necessary to suppose that the human intellect, instead of being, as it is, far stronger on the practical and inductive side than on the theoretical and deductive, so that practice usually precedes theory, had been stronger on the theoretical than on the practical side and that in 1858, when the theory of Natural Selection was enunciated, the practice of domestication of plants and animals or rather, let us say, their improvement by selection, had not been begun. What would biologists then have said? Clearly they would have reasoned: "If this theory be true; if nature have indeed raised up highly developed and specialised kinds of life from the simplest or from comparatively simple forms by destroying out of each generation the weaker members and reserving the stronger to continue the race; if plants and animals differ in their fitness to cope with their surroundings and it be on the average the fitter that survive and multiply, transmitting their superior fitness to their descendants: then man too, in his comparatively limited way, even in the short time at his disposal, must be able to produce proportionate results. Therefore, if we breed our cows only from the best milking cows and from bulls that are proved sires of good milkers, if we set aside exceptionally large-grained specimens of wheat as seeds for succeeding seasons, we shall be able to improve both cattle and wheat, slowly no doubt but to an indefinite extent in the selected characters. The experiment is doubtless absurd but it is harmless and the failure to produce results, say in the course of a century, will go some way to disprove the theory and clear the air of this crack-brained and pernicious nonsense." The proposal would

probably at first have been laughed out of court but afterwards it might have been tried and would have met with an unexpected degree of success; and this would have been experimental proof of the theory. Now, as the fact happens, it is just this sort of experiment that has for ages been extensively and continuously carried on by man in the process of domestication, that word being used in its widest sense to include the cultivation of plants. In what way and to what extent is the logical value of this series of experiments affected by the fact that it began long before Darwin was born? I venture to think it is not affected at all.

So far as can be done in a few paragraphs, it may be well to inquire in more detail what that process is and what it proves. Its origin, deeply buried in antiquity, is to us mere matter of surmise. It seems likely that it began not in any deliberate subjugation of animals by men but in a partnership due to mutual advantage. Probably wolves began to domesticate themselves with man as partners in the chase and scavengers to pick up the offal and bones after that clever hunter had gorged himself on his quarry, whilst men may have made a practice of following the pack in full cry and coming in at the death to rob them of their prey. Both practices would surely tend to the evolution of the friendly dog out of the unfriendly wolf, by the continual elimination of all such fiercer members of the pack as turned on men or refused to give them way. But however probable this may be, it is clear that no such surmises can be cited as experimental proof of the theory, simply because of the absence of all record of the facts. It is of the essence of such proof that it should be founded not on surmise however probable but on duly attested facts. Of these beginnings there are no records but as we travel downwards through history records begin to appear; first perhaps in the shape of wall-pictures and then in writings and finally in books, until at the other end of the scale we reach such facts as are cited in the following passage taken from Weismann's *Evolution Theory* (English translation), vol. i. p. 38: "Darwin says 'The English judges decided that the comb of the Spanish cock, which had previously hung limply down, should stand erect and in five years this end was achieved; they ordained that hens should have beards and six years later fifty-seven of the groups of hens exhibited at the Crystal Palace in London were bearded.'" What is proved by this double set of experiments or experiences? Among others

three points may be set down as well established: (1) The variability of certain characters of the so-called Spanish variety of the species *Gallus bankiva*, namely the comb in the male and the beard in the female. (2) That this variability is largely independent of the other characters of the variety. These appear to have been little if at all affected by the modification of the chosen characters. (3) That the variations can be accumulated in the same direction through several successive generations. The large number of persons engaged in the double series of experiments places these results of their concurrent testimony beyond doubt. Putting them together we may say that they prove the independent and cumulable variability of two particular characters of a particular variety of a particular species. If, however, these experiments are taken, as they must be, with hundreds of other series of experiments undertaken by other breeders by which they effected changes in other characters of the same variety of fowl, it will be seen that similar truths have been established in regard to a great number of them. And if these experiments again are taken with the experiments of thousands of other breeders of various varieties of the same species, it will be seen that the evidence of the independent and cumulable variability of at all events every conspicuous character of the domestic fowl is immense. Lastly *Gallus bankiva* is not the only species which has been modified by domestication into divergent varieties. If with the foregoing facts we consider the great number of animals and plants in regard to which similar truths have been established by similar experiments, the total evidence of independent and cumulable variability in the characters of organic beings becomes enormous and affords the best possible ground for the belief that the rule applies to every part of organic nature as a whole. The best possible; for what more can be proved by any expressly devised experiments in the case of species still undomesticated? Only that the rule applies to yet one more species or rather to one or more of its characters; a difference in the quantity of proof, not in its kind. Now to prove the existence of such variability throughout organic nature is to establish Darwin's law, at least as far as it can be established by experimental proof. For that it is in fact only or chiefly by this means that life as we now see it has been evolved, can be proved if at all only by appeal to the geological record; it is matter of inference from observation and not susceptible of experimental

proof. For my part then I cannot see that we have not here a proof of the theory just as complete as if it had been devised by scientists with all possible precautions and "controls" for the express purpose of scientific demonstration. Indeed the process must be much more conclusive than most experimental proofs, on account of the enormous number and variety of instances in which it has been tried and not found wanting. Those who differ from this opinion may fairly be challenged to devise and describe fully a crucial experiment or series of experiments which shall finally prove or disprove the theory; there will then possibly be found those who can spare the time and the means to put it into practice.

Suppose however it should be held that the interpretation I have here given of the word experiment is too wide and that to constitute an experiment properly so called definite and conscious purpose is a requisite, what then? In that case all breeding and nursery gardening carried on since 1858 by intelligent breeders and nurserymen who had read their Darwin, with the deliberate purpose of improving their stock, must still be regarded as experimental proof of the theory; for it cannot surely be vitiated as such by the fact that their chief purpose has been to profit by the sale of improved stocks and strains. The experimental production of artificial diamonds in proof of a theory as to the manner of their formation would not be held any less conclusive on account of the hope of the experimenters that a valuable product would be obtained.

Such considerations incidentally go to show the profound unreason of much of the early criticism of Darwinism. Darwin argued mainly from the phenomena of domestication in plants and animals (although indeed he also availed himself of all that was known of them under natural conditions) that species were proved to be artificially modifiable by means of selection for the purpose of reproduction of slightly superior plants and animals and must therefore also be modifiable and have been accordingly modified in a state of nature, unless we were to suppose that all the individuals of each species were of exactly equal fitness to cope with their environments. Oh but, it was replied, you cannot argue from the artificial conditions of domesticated animals to their conditions in a state of nature. That, said the then Duke of Argyll, is a "loose analogy." Man, was his unexpressed assumption, is so much more powerful than Nature

as to effect, in a few centuries, what Nature could not do in the course of geological ages ; all sorts of things can be done by art which are not done by Nature. He might just as well have argued that the blue crystals, artificially produced from copper and sulphuric acid, prove nothing regarding the behaviour of such materials in Nature ; or he might even more plausibly have asked what inferences could be drawn regarding natural phenomena from the liquefaction of hydrogen under conditions of cold and pressure which are not met in the natural world. The truth is that this distinction between the natural and the artificial, though no doubt it has its proper uses, is itself one of the most artificial things in Nature and in matters biological is often quite out of place. For what after all is man with all his works but a part of Nature ? Those, no doubt, who with Dr. Wallace at their head believe that at a certain stage of his development a spirit must have been breathed into an inhuman ape independently of the course of evolution in order to make him man, may logically dispute this conclusion, as man's mind in that case clearly contains a supernatural element, which must also have had its effect upon all his works, so that neither he nor they are entirely a part of the natural world. But those who see in the human mind nothing but a development, however great, of powers and faculties well indicated in the higher animals, will readily agree that he is a part of Nature and nothing more. Therefore as regards the evolution of animals and plants, he is merely a more or less important part of the environment ; to large animals a feature of ever-growing importance—to too many kinds, it is to be feared, the sinister omen of impending extinction ; to domestic plants and animals the dominating feature of their surroundings and factor of their lives ; but to deep-sea fishes a thing of remote if any consequence. When the flat-footed ape appears on the scene or at all events with the advent of the lethal variety called civilised man, the environment of large animals undergoes a great and rapid change and the qualities which before ensured their survival become comparatively useless. Strength, speed, wariness and ferocity now avail them little. Either they must accommodate themselves to his purposes and become domesticated or they must conceal themselves successfully to save their skins or they must perish utterly—*manet sors tertia cædi*. The presence of man radically alters the environment and

therefore the conditions of survival and the qualities required to secure it but that is all it does. Neither the animals that accept man's yoke nor those that survive in spite of him are withdrawn from the realm of nature or escape her law. Her writ runs in byre and garden as well as in forest and plain. The argument from domestication was not therefore an analogy at all, still less a loose one. Improvements of stock by selective breeding constituted in themselves the proof of the theory of selection by demonstrating both the variability of species and the fact that favourable variations do occur and are selected and by accumulation may result in great modifications of any part or character.

Not only so but in the phenomena of domestication it appears to me we have the only possible complete experimental proof of the theory, which therefore either has been or never can be experimentally proved. That control of organic beings which is requisite in order to constitute an experiment in the phenomena of reproduction can only be obtained by what amounts to domestication. The relation between observation and experiment is similar to that between nature and art, so that the element of art or artifice in domestication, far from vitiating its results as a source of inference, is precisely what makes them the proper material for final and conclusive proof.

In a word, man being a part of Nature, selection by man does not merely prove but *is* natural selection and we *have* "seen natural selection at work." Nature acting by man's own hand long ago began and has since in an ever-increasing degree continued to select for survival those plants and animals which are useful or pleasing to her simian pet and to destroy his enemies.

Let us now turn to some fresher expression of opinion on the subject than that above selected. "The theory" (of natural selection), say Profs. Geddes and Thomson in their recent popular hand-book on Evolution, "works well as an interpretation but what we need is actual proof of discriminate selection, actual evidence that survivors do survive in virtue of particular qualities." Do not many cows survive in virtue of the particular quality of giving a good supply of milk and many go to the butcher by reason of their failure to do so; have we not here actual proof of discriminate selection? As partly satisfying their demand, the Professors go on to describe an

experiment made by Mr. A. P. di Cesnola, who having exposed some dozens of the two forms of Italian Mantis, green and brown in colour, some in herbage which matched their colouring and others in herbage which did not, found that the latter were soon taken by birds whilst the former were left. Thus the survival value of the protective colouring was distinctly proved but not its cumulative inheritance from generation to generation nor the variability of the species nor the survival value of small differences, of all of which we have ample proof in the phenomena of domestication. As to the point that is proved, far be it from me to detract from the cogency of the proof but why is it more conclusive than any one of an infinite number of experiments tried by humanity during hundreds, if not thousands, of years by which they have unintentionally demonstrated the survival value of this very character of colour in other ways? Colours have been points selected by breeders and gardeners in the case of cattle, dogs, pigeons and numerous flowers during centuries. In the one experiment, a few dozen Mantis were demonstrated to have survived by virtue of colours corresponding with their surroundings; in the others, millions of plants and animals have survived and have been selected as progenitors of the future race by virtue of colours corresponding with preconceived ideals of beauty in the minds of men. Nature in both cases is the selector; in the one case her selective agents were birds, in the other case men. In either case has the possession of a particular colour been a favourable variation determining the survival of a particular animal or plant in competition with his fellows less fortunately endowed. Why is the single experiment more cogent than the million?

Again Dr. G. Archdall Reid is one of the ablest of present-day exponents of organic evolution especially in relation to man and his treatment of the subject of elimination by disease ought to have and doubtless has gone far to dissipate the dense fog of much loose writing on the supposed immunity of modern man from Natural Selection. Yet he writes (*Bedrock*, No. 2, p. 262): "It is necessary . . . to ascertain whether Natural Selection does really occur in Nature, to observe what kinds of variation it selects and to discover the result, if any, of this selection. It is useless to observe domesticated plants and animals; they are under artificial selection." But why does the fact that they are under artificial selection make it useless to observe domesticated

species for this purpose? I have given above some grounds for thinking that it is precisely this fact that makes them the proper and the only possible material for experimental proof. Domesticated races have not been withdrawn by man from the operation of the active forces of organic nature. Her laws and methods of nutrition, growth and reproduction have not been essentially altered in their case. Had calves and puppies, peas and cabbages in civilised countries ceased to be products of Nature and become works of art like watches or pictures, such expressions would be justified but hardly otherwise. It seems to be forgotten that these species were wild before they were tamed and that some of them at least have congeners yet living in freedom from whom they have diverged under the control of man. Such divergence is proof of the variability of the wild species. In the course of some very effective criticism of Mendelism, Dr. Reid points out that that school has no monopoly of the method of experiment in the study of Biology and did not therein initiate its use, which was practised by Darwin and others before Mendelism was thought of; but if I may say so, he does not go far enough. Ages probably have passed since some one first consciously tried the experiment of breeding from the fastest greyhounds with the deliberate object of improving the race; ages again before that men unconsciously did the same thing by keeping the hounds they found most useful in the chase and destroying or neglecting the rest, so that as a fact this type of hound is said to be delineated in the wall-carvings of ancient Egypt. This attitude towards the argument from domesticated races seems the less defensible in Dr. Reid, because he justly insists that, as of all animals the best known to us is man, he is therefore the best subject for biological speculation. For the like reason, that we know far more of them than of wild animals and plants, domesticated species furnish the second best materials for experiment and research. Indeed, for the former purpose they are surely the more suitable, both because they are much more amenable to control and because of their far greater rapidity of reproduction. And it may be claimed that this view is supported by the facts, for after all it was from the domesticated races that Darwin chiefly drew the data upon which he founded and, whether by analogy or as I contend by proof positive, finally established his theory.

Upon the whole it seems that an incorrect and exaggerated

estimate of the scope and nature of man's interference by domestication in the process of evolution is widely current and finds a footing even among the most enlightened evolutionists. Theoretically Darwinism has put man in his proper place in the world and killed the anthropocentric theory but in practice the anthropocentric habit of mind dies harder and its vestiges remain in our brains in spite of ourselves and influence thought unawares. It seems to be vaguely supposed or unconsciously assumed that by domestication a species is removed from the operation of natural law but properly regarded domestication is nothing but a radical alteration of the environment, in which a new set of qualities, including some and excluding others of the old set, constitute fitness and secure survival. Beyond confinement and slaughter man does nothing but select the variations which Nature, constant in nothing but change, invariably presents. We may consider domestication broadly as a kind of symbiosis, comparable with though widely differing from other kinds occurring lower in the scale of life, in which two species find it to their common advantage to live in close companionship. If it be objected that, in this case, the advantage is one-sided, the answer is that the domesticated beast secures at least the main advantage of nutrition and reproduction, whilst the cultivated plant may be said to secure everything it would wish, if it could wish for anything. Like enlightened merchants, they have found their own advantage in supplying the needs of others; or perhaps they are more like the unenlightened, who do so unconsciously or even in spite of themselves. And another answer is that there is no law of nature that in symbiosis the advantages of the partnership must be equal. Parasitism may be considered as a kind of symbiosis in which they certainly are not so.

But, it may be said, breeders have never formed two species out of one but only varieties which are always capable of interbreeding. The objection would have more weight if any one could tell us what a species is. It is not denied that the differences between domestic varieties of dogs and pigeons are far more than enough to have constituted them separate species or as some say even genera, if they had been found in a state of nature; whilst as for sterility, there are plenty of hybrids to prove that it is not an essential but only an accidental feature of natural species; and on the other hand, Darwin gave evidence.

of the occurrence of sterility between varieties, evidence which he considered it "impossible to resist." There is therefore, in point of sterility, no real distinction between genus, species and variety and the objection fails. The classification of animals and plants depends or ought to depend always on the number and extent of the differences in that assemblage of characters which constitutes the organism as a whole, the degree of sterility constituting only one difference among many.

It is probably in Darwin himself that the original source of the error is to be found and I may fitly close my argument with a condensed quotation from the *Origin of Species* which should, I think, at the same time effect the final removal of any obscurity about the point I have endeavoured to establish. The passage occurs in Darwin's exposition of the principle which he "called for the sake of brevity 'Natural Selection,'" in the summary of the fourth chapter: "If organic beings vary at all in the several parts of their organisation and if there be, owing to the high rate of increase of each species, a severe struggle for life at some age, season or year, it would be an extraordinary fact if no variation ever had occurred useful to each being's own welfare in the same manner as so many variations have occurred useful to man. But if useful variations do occur, assuredly individuals thus characterised will have the best chance of being preserved in the struggle for life and from the strong principle of inheritance they will tend to produce offspring similarly characterised." I need hardly say, I do not quote this most moderate statement for the purpose of dissent but my comment is this: that the "many variations" which "have occurred useful to man" in domesticated plants and animals *are* by that very fact "variations useful to each being's own welfare," since they *have* given "the individuals thus characterised the best chance of being preserved," as is shown by the fact of their preservation and such individuals *do* "produce offspring similarly characterised," so that the variations can be and have been accumulated from generation to generation to produce an indefinite amount of change. If this be so, the preservation of favoured races in the struggle for life by means of Natural Selection and the consequent production of new and more specialised forms widely differing from the old is not a theory but an experimentally proven fact.

THE MEASUREMENT OF OSMOTIC PRESSURE BY DIRECT EXPERIMENT

By T. MARTIN LOWRY, D.Sc.

A. OSMOSIS AND OSMOTIC PRESSURE

As long ago as 1748 it was discovered by Nollet that a flow of water took place through a membrane of pig's-bladder separating alcohol from water. This observation was forgotten during more than half a century, until it was redescribed in 1802 by Parrot,¹ who also detected a similar flow when urine was used instead of alcohol. Parrot recognised that a flow of liquid took place simultaneously in both directions but that the velocities differed so widely that a pressure might be developed, on one side of the membrane, equivalent in some cases to a column of water not less than 10 ft. in height. Quantitative measurements made by Dutrochet (1827), to whom we owe the terms exosmose and endosmose and by Vierordt (1848) showed that the rate of flow depended on the nature of the membrane, on the concentration of the solution and on the temperature; but the factors determining the flow were too complex to allow of any simple statements of the laws governing osmosis. One of the first generalisations to be attempted was suggested by Jolly in 1848, when he brought forward evidence to show that a fixed ratio existed between the exosmosis or outward flow of the salt through the membrane and the endosmosis or inward flow of water into the solution. This ratio, the "endosmotic equivalent," he supposed to be independent of the concentration but further investigation showed that this was not the case.

Equally little progress was made when experiments were carried out to determine the maximum "head" of liquid which could be driven up by the osmotic flow of water into a solution. It is true that one factor, the frictional resistance of the membrane to the endosmotic flow, was now eliminated; but so long as an exosmotic flow still took place the "head" of

¹ See Walden, "Die Hauptdaten aus der Geschichte des Osmotischen Drucks und der Osmotischen Lösungstheorie," *Bull. Acad. Sci., St. Petersburg*, 1912.

liquid or "osmotic pressure" was still dependent on the individual properties of the particular membrane used. No real progress could be made until this difficulty was overcome by the discovery of "semi-permeable" membranes which would stop completely the outward flow of the solute whilst still permitting the solvent to pass inwards to the solution and there develop the maximum osmotic pressure that was possible. Such membranes were, in fact, discovered by Traube in 1865 in the form of floating films precipitated by the interaction of two contiguous solutions. Traube then showed that if solutions of copper sulphate and potassium ferrocyanide are brought together, a floating membrane of copper ferrocyanide is produced which is permeable by water but impermeable by both salts. According to the relative strengths of the two solutions, water is drawn in one direction or the other through the membrane which is so displaced that it always forms the boundary between the two solutions. If the boundary expand or if the membrane be broken, a fresh precipitate is at once produced by the interaction of the two membrane-forming solutions.

But whilst Traube's membranes possessed the property of being semi-permeable, they were not suitable for quantitative experiments, as they were incapable of supporting even the smallest osmotic pressure. Great importance attaches therefore to the introduction by Pfeffer in 1876 of methods by which Traube's membranes could be strengthened by precipitating them on linen or silk or parchment or best of all in the pores of an unglazed porcelain battery-jar. With this equipment, it was possible, for the first time, to make real measurements of the maximum osmotic pressure set up in a solution by the inflow of water through a semi-permeable membrane. Even then, however, very few regularities were discovered: the maximum pressure was found to be proportional to the concentration of the solution but no indication was obtained of any law by which the magnitude of the pressure could be predicted.

B. VAN'T HOFF'S EQUATION

In view of the obscurity in which the phenomena of osmosis were involved, it would be difficult to exaggerate the dramatic effect produced by the discovery, made by Van't Hoff in 1887,

that the gas-equation $PV = RT$ could be applied directly to solutions, if "osmotic pressure" were substituted for "gas pressure." This remarkable generalisation appeared to illuminate a vast range of difficult and puzzling phenomena and at the time of its introduction it was widely believed that the problems of osmotic pressure and of solutions had for the most part been finally solved.

Van't Hoff's conclusions were based on the measurements which had been made by Pfeffer in the botanical laboratory at Bonn about the year 1876. They were supported by a consideration of cognate properties, such as the lowering of vapour pressure and the depression of the freezing-point in solutions, properties which had been studied by Raoult which were now shown to be related thermodynamically to the osmotic pressure. Using the somewhat scanty data then available, Van't Hoff showed that Boyle's Law could be applied to solutions, since (as Pfeffer had found) the osmotic pressure was proportional to the concentration of the solute and therefore inversely proportional to the volume to which it was diluted in the solution. He next discovered the fact (which had been overlooked by Pfeffer) that the small temperature coefficient of osmotic pressure is identical with the corresponding coefficient in gases, so that osmotic pressure is (like gas-pressure) directly proportional to the absolute temperature. Having thus proved that osmotic pressure could be expressed by the equation $PV = RT$, he calculated from Pfeffer's data the value of the constant for a gramme-molecular proportion of sugar and found that it was identical with the constant of the gas-equation $PV = RT$. This equation could therefore be used equally well to calculate the pressure of a gas or the osmotic pressure of a solution.

The validity of the equation in the case of solvents other than water and of solutes other than sugar was deduced from the substantial identity, in the case of twelve solvents, of Raoult's "Molecular lowering of the vapour pressure" with figures calculated from the formula $K = M/100$ (K = molecular lowering, M = molecular weight) and of Raoult's "Molecular depression of the freezing-point" with figures calculated from the formula $t = 0.02T^2/W$ (t = mol. depression, T = abs. temp. of f.p., W = latent heat of fusion) in the case of five solvents. As both formulæ were based on the assumption that osmotic pressure

obeyed the gas laws, the agreement afforded further proof of the numerical agreement between the two sets of phenomena.

It must now be admitted that the evidence on which van't Hoff's magnificent generalisation was based was of a very inexact character. Thus, whilst Pfeffer's observations showed a general tendency for osmotic pressure to increase with rising temperature, the individual figures pursued a zig-zag course, departing (in a range in which the whole change of osmotic pressure was only 10 per cent.) by as much as 3 per cent. from the smoothed values calculated by van't Hoff. Again, in using Raoult's "molecular depressions of the freezing-point" as confirming his laws of osmotic pressure, van't Hoff's figures showed deviations up to 6 per cent., whilst the values for the "molecular lowering of vapour pressure" showed differences up to 10 per cent. between the observed and the calculated values. The situation presents, indeed, many similarities to the circumstances under which Dalton promulgated his atomic theory on the basis of data so inaccurate that he was able to recognise the presence of a single equivalent of nitrogen both in nitric oxide and in ammonia, two compounds in which the actual proportions differ no less than 50 per cent.! But in each case the generalisation was so bold and far-reaching that its inherent truthfulness was at once recognised, in spite of the inexact character of the evidence which could be produced in its support. In the case of the atomic theory, Berzelius and Stas carried out series of exact measurements which established beyond all question the validity of the atomic theory as an accurate expression of the laws of chemical combination. In the case of van't Hoff's generalisation, measurements of similar exactitude, made by Griffiths at Cambridge, proved that the formula could be applied accurately to calculate the depression of the freezing-point of water by cane sugar and by potassium chloride at extreme dilutions. But all the accurate measurements of osmotic pressure that have since been made have gone to prove that, whilst van't Hoff's law may give an exact representation of the properties of very dilute solutions, it fails utterly to express the properties of solutions of even moderate concentrations and is of value mainly in providing a base line for the study of the deviations which they exhibit from the requirements of this law.

The exact measurement of osmotic pressure is therefore

a matter of very great importance both in order to determine the actual magnitudes of the pressures and in order to provide data for a theory of solutions which shall be applicable under conditions other than those of "infinite dilution."

It may be asserted emphatically that nothing, at the present time, can take the place of direct measurements of osmotic pressure carried out with the greatest care and exactitude. Calculation fails utterly to represent the observations that have been made: attempts to substitute indirect measurements for direct measurements are almost equally useless: firstly, because calculations are required which often involve approximations or the use of constants of doubtful accuracy; secondly, because it is impossible to make isothermal measurements of the freezing-point or boiling-point of a series of solutions, whilst vapour-pressure measurements although made isothermally are usually far from exact.

The foregoing statement will serve to explain the great interest and importance which attaches to the exact measurements of osmotic pressure which have been made during the opening years of the present century by the Earl of Berkeley and his colleagues in England and by Prof. H. N. Morse and his colleagues in America. The American work, in its general features, follows the methods used a quarter of a century before by Pfeffer and will be described as a sequel to his work; but ten years of laborious experiment were required before all the main sources of error were eliminated: the measurements extend from decinormal to normal concentrations, whilst the range of pressures is from 2 to 25 atmospheres and the range of temperatures from 0° to 80° C. The measurements of the Earl of Berkeley and Mr. E. G. J. Hartley, which extended the range of pressures up to 135 atmospheres, were carried out with a novel type of apparatus, which will be described most conveniently in the later part of the present article.

C. PFEFFER'S EXPERIMENTS

The experiments described in Pfeffer's *Osmotische Untersuchungen* (Leipzig, 1877) cover a very wide range of phenomena. Observations were made of osmosis through membranes of many kinds; some of them were permeable to the solute as well as to the solvent, others were permeable to the solvent only. Experiments were made both on the rate of

osmosis under different conditions and on the maximum pressure that could be set up by the osmotic flow. The most important experiments were those in which this maximum osmotic pressure was measured, using as a "semi-permeable" membrane the precipitated films of copper ferrocyanide first described by

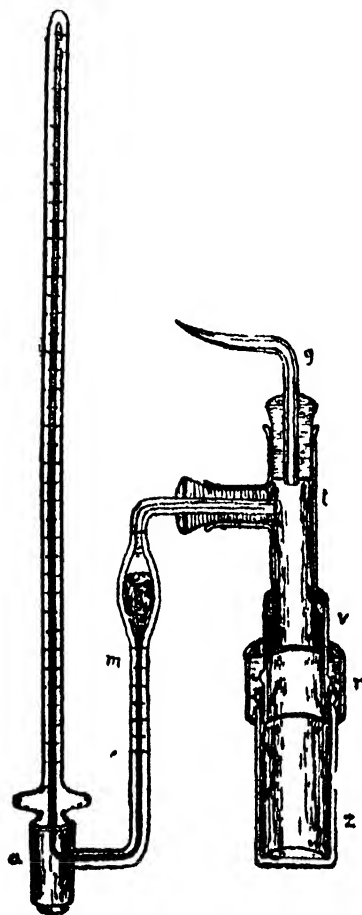


FIG. 1.

Traube in 1865. These measurements were carried out with the cells shown in fig. 1.

Rigidity was conferred upon Traube's floating membranes by depositing them first upon linen or silk but finally in the walls of a porous battery-jar, a method that has been in use through thirty-five years of subsequent work and has been proved to be capable of furnishing membranes strong enough to resist

pressures up to 150 atmospheres.¹ The porous pot *z* was 46 mm. high and 16 mm. wide with walls $1\frac{1}{4}$ to 2 mm. thick. The glass tubes *v* and *t* were joined to the porous pot by two layers of shellac, the upper hard, the lower a little soft, in order to make a sound joint. The softening of the shellac at higher temperatures (up to 37°) was compensated by the addition of a glass ring *r* filled with cement which held the apparatus rigidly together and by a layer of the same cement above the shellac in the joints between *v* and *t*; the cement used was the well-known mixture of litharge and glycerol. Before depositing the membrane the porous pot was extracted with potash and with chlorhydric acid and freed from air by soaking in water and evacuating with an air-pump. The pot was soaked during several hours in a 3 per cent. solution of copper sulphate, rinsed internally with water and partially dried with filter-papers and by exposure to the air, then filled with a 3 per cent. solution of potassium ferrocyanide and immersed again in the copper-sulphate solution. After standing during twenty-four to forty-eight hours the cell was closed and exposed to the pressure due to the osmosis of the membrane-forming solutions; twenty-four to forty-eight hours later it was emptied, charged with a 3 per cent. ferrocyanide containing $1\frac{1}{4}$ per cent. of saltpetre and exposed to the osmotic pressure of 3 atmospheres which this solution develops or if necessary to the higher pressure developed by a stronger solution. Membranes of Prussian blue were prepared in the same way by using $1\frac{1}{4}$ per cent. ferric chloride outside and 3 per cent. ferrocyanide inside the cell, whilst membranes of calcium phosphate were prepared from a 3 per cent. solution of calcium chloride and a 6 per cent. solution of disodium phosphate neutralised with sodium bicarbonate. Membranes of ferric hydroxide and ferric phosphate were also tried.

It will be seen from the description that has been given not only that Pfeffer's experiments were very extensive in their range but that they were carried out with very considerable care. It is, indeed, noteworthy that his methods were adopted almost *in toto* by Morse twenty-five years later and that nearly all the sources of error which the American workers strove so long and so successfully to eliminate had been recognised (and

¹ Pfeffer records pressures up to 436.8 cm., *i.e.* nearly 6 atmospheres, in the case of a 3.3 per cent. solution of saltpetre in water,

to some extent guarded against) by Pfeffer; in particular, the German botanist was aware of the errors due to leakage of the solution through the membrane and to dilution of the solution by inflowing water; he saw the importance of using a manometer of small bore and stoppers of slight compressibility in order to diminish the inflow and actually invented the method of applying pressure mechanically in order to reduce this factor to the smallest possible dimensions.

The osmotic pressures developed in the apparatus were measured by means of an air-manometer (fig. 1); this had a closed limb graduated over a range of 200 mm. and a short open limb also graduated from the same zero and provided with a bulb to act on a mercury reservoir. In order to secure rapid adjustment, the bore of the tube was small, about 1.2 mm.; the air was renewed after every five experiments lest water should have crept into it; a joint at *a*, by which the long limb could be disconnected, also served as a tap by which the manometer could be cut off from the osmotic apparatus.

As is shown in the figure, the final sealing of the apparatus, after it had been completely filled with solution, was effected by fusing the capillary point of the glass tube shown at *g*; the tube *g* was then forced down a little, in order to hasten the attainment of a steady pressure and reduce the quantity of water entering the cell. The inflow of water into the most concentrated solutions, due to the displacement of 100 mm. of mercury, was about 0.11 c.c. on a total of 16 c.c.; the compression of the rubber stoppers amounted to 0.05 c.c. at 2 atmospheres and 0.09 c.c. at 4 atmospheres; but the total inflow can scarcely have exceeded 0.14 c.c. or less than 1 per cent. when the glass tube *g* was pressed down after sealing.

The rubber stopper holding the tube *g* was wired down when using higher pressures up to 7 atmospheres. Steady conditions of temperature were secured by immersing the whole apparatus in water or in a dilute solution of a membrane-former; thus a 0.09 per cent. solution of copper nitrate was often used, a 0.1 per cent. of ferrocyanide being placed inside the cell to balance it.

The concentrations of the solutions were checked by measuring their densities both before and after they were used for the osmotic experiments; in the case of sugar solutions the polarimeter was used to check both the concentration and the purity of the sugar. The substances examined were cane sugar,

gum arabic, dextrin, cream of tartar, Rochelle salt, saltpetre and potassium sulphate.

D. MORSE'S EXPERIMENTS

The experiments on osmotic pressure which have been conducted at the Johns Hopkins University by Prof. H. N. Morse and his co-workers have formed the subject of twenty-five papers published in the *American Chemical Journal* from 1901 to 1911. But most of the essential features of the earlier papers are described, with methods perfected and data corrected, in a series of five papers which appeared in that journal in 1911 under the heading "The Relation of Osmotic Pressure to Temperature." These five papers will long stand as one of the monuments of Physical Science and may already be ranked with the great classics of earlier generations. A sixth paper dealing with the "Osmotic Pressure of Cane-Sugar Solutions at High Temperatures" has appeared during the past year and a further paper on this subject is promised. It will be convenient to describe in series the chief features of the apparatus which enabled the American workers to reduce the measurement of osmotic pressure from a rough approximation to an exact routine.

1. *The Manufacture of the Cells.*—One of the most serious difficulties in the measurement of osmotic pressure is to secure suitable porous pots. This difficulty was encountered by Pfeffer but became of dominant importance in the more exact work of the American investigators. At the end of four years they had secured (from a batch of 100) only *two* cells with which they could measure osmotic pressure with some degree of confidence, whilst 25 or 30 answered the requirements moderately well. A whole year spent in procuring and testing nearly 500 more cells from different makers revealed *not one* that was suitable for the work and showed that the problem must be transferred from the pottery to the laboratory.

The chief faults of the commercial cells were :

- (1) Insufficient strength : only a few survived 30 atmospheres, whilst most of them cracked at pressures below 20 atmospheres.
- (2) "Air-blisters," communicating with each other and with the interior of the wall, which gave rise to a series of subsidiary membranes in the interior of the wall.

- (3) Unequal porosity even in the same cell, which caused the membrane to wander towards the outer wall at every locality of coarse texture.

The problem of making cells in which all the essential qualities should be combined was finally solved by avoiding altogether the use of ground feldspar as a binding material and selecting as raw materials two natural clays, one deficient in binding material the other over-rich in that constituent; these could be mixed very intimately and never failed to give products which were perfect in respect of uniform porosity. The carefully prepared mixture was packed into a cylindrical steel mould and subjected during fourteen to sixteen hours to a total pressure of about 200 tons. From these cylinders cells were turned out on the lathe, both the chuck and the cutting tools being of special design; the difficulty of this operation is shown by the fact that at first 90 per cent. of the cells cracked in the kiln, a proportion that has now been reduced by careful working to about 10 per cent. After baking at about 1300°C . the pots were ground to take the metal fittings and then glazed inside and out, from the middle upwards, with a special glaze prepared by adding silica and feldspar to one of those used by potters for the better grades of white tableware.

Fig. 2 shows the complete cell as fitted up for use at the present time. The cell and the manometer are clamped together by means of a brass collar (1) and a brass nut (2), the washer (3) being made of lead. The main brass cone (4) is pierced with two holes for the manometer tube (5) and for the hollow needle (6), which are both secured by means of Wood's metal at (7) and later by a cone of the same metal at (11). The joint between the metal fittings and the pot is made by means of a rubber tube (12) wound tightly at the upper and lower ends with twisted shoemakers' thread (13, 14). The hollow needle (6) is nickel-plated and brazed into a brass piece (8), which is bored and threaded to fit the closing-plug (9); the grease-filled leather packing at (10) makes a tight joint when screwed down.

2. *The Manometers*.—The form of manometer used in all the later experiments on the influence of temperature on osmotic pressure is shown in fig. 3. The bore is very small, from 0.45 to 0.72 mm. The advantages of narrow tubes are

- (a) that the short mercury columns at the top of the capillary are less liable to be displaced by tapping;

- (b) that the compression of the small volume of mercury which they contain involves but little dilution of the contents of the cell;
- (c) that only small volumes of the specially purified mercury are required.

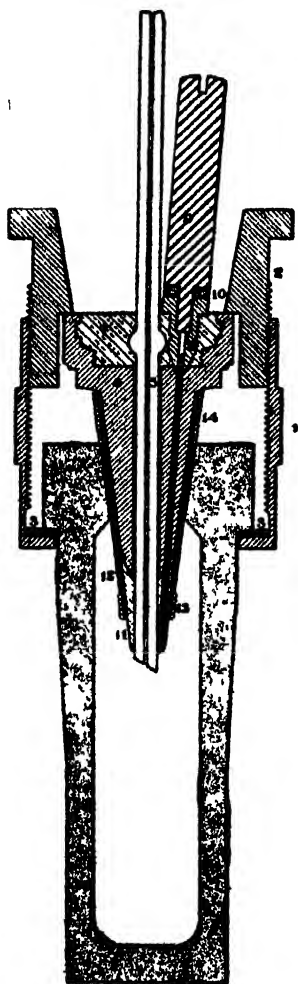
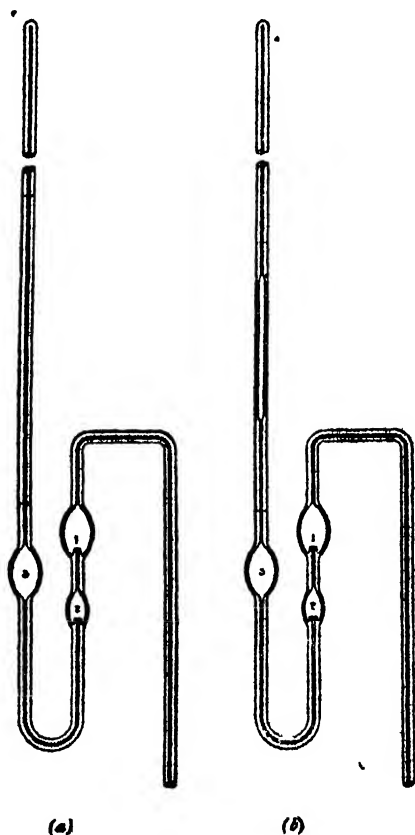


FIG. 2.—Osmotic cell complete.



(a) For moderate pressures.
 (b) For high pressures.

FIG. 3.—Manometers.

On the other hand :

- (d) the meniscus is more troublesome ;
- (e) the capillary depression is large and varies so greatly with the bore of the tube that it can only be determined by direct calibration ;

(*f*) the movement of the mercury is much influenced by impurities in the mercury or attached to the surface of the glass.

An improved manometer, specially suitable for measuring large pressures, has a capillary which is enlarged in the lower part of the tube to sixteen times the normal sectional area; this leaves a much longer column of gas to be measured at the higher pressures but has not been used in the present series of measurements. The bulb (3) is intended to prevent the escape of nitrogen from the calibrated portion of the tube when the pressure is reduced; the traps (1) and (2) serve to catch minute particles of solution which are carried forward by the mercury during fluctuations of pressure; these traps effectually prevent the disaster which results when such particles work their way into the calibrated portion of the tube, compelling a dismantling and cleaning of the whole apparatus. The short column of mercury (4) at the top of the tube serves to prevent contamination of the nitrogen while the instrument is being closed and afterwards keeps the gas out of the portion of the tube the calibration of which has been affected to an unknown extent by fusing off the ends. Two very fine marks are etched on each manometer, one near the bottom of the calibrated portion of the instrument and the other higher up: these are the only reference lines, as any attempt at graduation would interfere with the accurate location of the meniscus. The distance between the two marks is known, so that when one is out of sight in the bath, readings can be taken from the other line and then referred back to the first.

The tubes are selected from large batches of the best commercial qualities, one end of each selected tube being marked and cut off so as to be available for sealing on to the manometers if and when required. The tube is then calibrated, either in a vertical or in a horizontal position or both, by means of (*a*) a short thread of mercury of known weight, which is measured in a series of positions along the tube and (*b*) a long thread which fills the tube between the reference marks near the ends of the tube and which is also weighed. The two weighings do not give the same figure for the weight of mercury per millimetre of the tube because of the curvature of the meniscus; but from the difference the volume-error due to the meniscus can be calculated and applied to the subsequent readings of

the manometer. The meniscus error thus determined is only about three-fourths of that calculated for spherical surfaces; this may be due to the actual shape of the meniscus or perhaps to a tendency to read the columns too short; in either case the same factors would probably appear in the reading of the manometers and would be eliminated by taking the corrections as found experimentally rather than by calculation. The correction for the meniscus amounts to 0.141 per cent. in decinormal solutions increasing to 1.07 per cent. in normal solutions; but it is believed that the difference of 25 per cent. between the experimental and the calculated corrections is much greater than the actual error in this correction, in any case the meniscus error is insignificant when dealing with temperature coefficients.

The capillary depression of the mercury was determined by direct comparison of the readings in the tube with those of a wide tube into which mercury was driven up from the same reservoir. The correction amounted to as much as 18 mm. or 0.023 atmosphere and was one of the most fertile sources of error, since no relationship could be traced between the variations of capillarity and variation of bore. The same apparatus was used to determine the volume of purified nitrogen finally introduced into the manometer tube after sealing on to the bulbs, etc., shown in fig. 3. In each case it was found that increased errors appeared when using a calibrated tube or manometer as a standard for direct comparison: in this case the readings were affected by errors due to the irregular capillarity in both instruments and it was found desirable (in spite of the increased labour involved) to regard each manometer as an independent standard. The labour involved in this essential and difficult work is illustrated by the statement that "the whole time of one of the authors of this paper is given up to the study of the manometers which have been or are to be used in our measurements of osmotic pressure."

3. *The Regulation of Temperature.*—Questions of exact regulation of temperature are of altogether exceptional importance in the measurement of osmotic pressure. In nearly every kind of physical work it is sufficient that uniformity of temperature shall prevail throughout the apparatus at the moment when the readings are taken. But in dealing with osmotic pressure any temporary fluctuation of temperature produces

effects which may persist during many hours or even days after the temperature has again been brought under control. This must necessarily be the case, since the cooling of the cell diminishes the volume of the contents, reduces the internal pressure and permits water to enter the cell, thereby causing a local dilution which may persist during several days. Conversely, if the cell becomes heated when a condition of equilibrium has been attained, the expansion of the contents will drive water from the cell and concentrate the solution; if the dense concentrated liquid should sink to the bottom of the cell, much time must be allowed for it to rise again by diffusion and ultimately regain its normal concentration.

Similar conditions prevail when the cell is first closed. Not only must pressure (approximately equal to the osmotic pressure expected) be applied immediately to prevent water from entering the cell and diluting the contents but this must be done at the right temperature. The whole of the apparatus, solutions, water, etc., must therefore be kept in a thermostat in readiness for setting up.

The "thermometer-effects" due to fluctuations of temperature were eliminated by using a series of thermostatic devices to control the temperature of large water-baths and air spaces. These were all constructed on one common principle: water or air is passed rapidly (1) over a continuously cooled surface, then (2) over a heated surface which is more efficient but is under the control of a thermostat, (3) thence into or around the space occupied by the apparatus, again over the cooled surface and so on. Figs. 4 and 5, which show the thermostatic devices used in the actual measurements of osmotic pressure, are typical of a dozen such baths used for various purposes.

Fig. 4 shows the water-bath containing the cells. The cooling surfaces 3, 8, 7, etc., are supplied with water from the hydrant cooled, when necessary, before entering the bath by passing it through a coil immersed in ice. The heating surfaces 9 and 10 contain sockets for four lamps the current through which is controlled by the mercury thermostat at 1. By means of the propeller shown on the left of the figure, water is drawn out of the bath through the pipes 12 and 13, brought back again through the pipe 14 and distributed through the bath; whilst outside the bath, in the short curved pipes leading from 12 and 13 to 14, auxiliary gas-heating can be

applied to the water, when working at the higher temperatures, leaving only a small balance to be provided by the regulated electrical heating. Ample provision was made for driving enough water through the bath to keep the temperature uniform from end to end: usually a circulation of 400 litres per minute was found to be ample.

Fig. 5 shows the arrangement of the air-space above the bath. This is provided with a system of pipes 7 through which cold water can be circulated, a system of pipes 8 through which either hot or cold water can be circulated and a series of four shaded lamps 3, 4, 5, 6, controlled by the mercury thermostat 10. A fan 9 driven by a motor provides

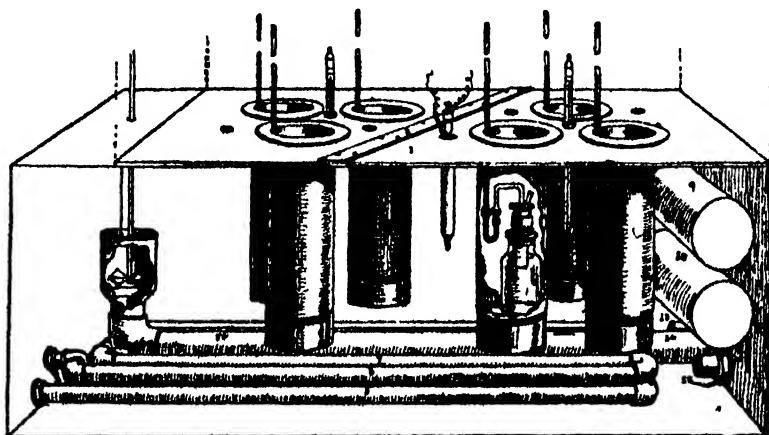


FIG. 4.—Thermostat containing osmotic cells.

a vigorous circulation of air and also serves to keep the manometers continually agitated.

Special apparatus has recently been introduced to secure a steady temperature exactly at 0° but this need not now be described in detail.

4. *The Membranes.*—The first membranes were formed in the interior of the cell-walls but it became clear that with a membrane so located it would not be possible to measure osmotic pressures. In such a cell the minute pores between the membrane and the inner wall would be choked with water, which would require very long periods of time before it could be displaced by the solution; moreover, any temporary dilution or concentration of the liquid in the pores, due to the displace-

ment of water through the membrane, would produce effects which might last for a very long time. It was therefore necessary to form the membrane on the inner wall of the cell, an effect which could easily be produced by diminishing the diameter of the pores. When, however, the texture was too fine the membrane was not satisfactory, probably because it was rooted with insufficient firmness to adhere properly to the surface: it was also difficult to develop a good membrane on a cell from which an old membrane had been cleaned off.

The first step in preparing the cell was to displace the air in the pores by water. This was effected by "electric endosmose."

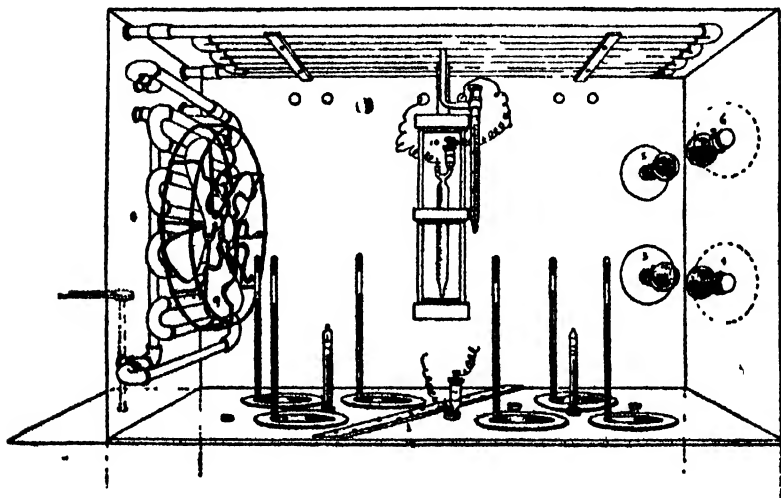


FIG. 5.—A r-space above cells.

The cell was filled with a 0.005 normal solution of potassium sulphate and was then immersed in a similar solution to the lower edge of the glazed portion. By passing a current inwards through the cell, water was drawn in continuously: the cell was then rinsed and soaked and the same process repeated with distilled water. In the later work, lithium sulphate was substituted for potassium sulphate, as it was found that "the quantities of water carried through the porous walls of a cell, under identical conditions, are inversely proportional to the relative velocities of the various kathions divided by their respective valencies."

To deposit the membrane, the cell was set up with a

cylindrical platinum kathode inside and a cylindrical copper anode outside. Simultaneously, the interior was filled with N/10 ferrocyanide and the outer space with N/10 copper sulphate. An electric current under a pressure of 110 volts was applied during two or three hours until a maximum resistance was reached, the interior being rinsed out with fresh ferrocyanide every two or three minutes to remove the alkali set free by the electrolysis. The cell was then rinsed and soaked during one to three days and the process repeated. It was found essential to deposit the membrane at a temperature not lower than that at which the cell was to be used; similarly it was advisable to measure the osmotic pressures first at higher and afterwards at the lower temperatures. The membranes were tested with weight-normal sugar solutions, with membrane-formers of N/10 concentration, the course of the meniscus in the manometer being carefully watched to detect irregularities of motion due to the breakage and repair of the membrane in the pores. The electrolytic treatment and tests were repeated over and over again until the behaviour of the cell was satisfactory, the membrane-formers in the osmotic tests being finally reduced to 0.01 osmotically normal concentration. The minimum time required to form a cell was a month but the operation often occupied three or four months, all the essential operations being carried out in thermostats. In testing the membranes it was not sufficient to secure steady pressures: no reliance was placed upon a cell in which the *maximum* pressure for the given concentration was not developed. When the cell had been passed as satisfactory, no experiment was accepted in which the concentration of the solution was not perfectly maintained. This was found to be no mere ideal but a test that could be applied rigidly to every measurement recorded. In one experiment, a cell, not specially selected, a constant pressure of 12.522 atmospheres was maintained *during sixty days*, the range of fluctuation being almost exactly equal to the range of atmospheric pressures during this period.

In a new cell the maximum osmotic pressure might be reached in as little as six hours; in an old cell, with a greatly thickened membrane, as much as ten days might be required. The old membranes were perfect in their osmotic qualities but were rejected because of their slow action. This rendered them tedious to use and greatly increased the lag in recovering

from "thermometer effects" and "barometer effects" due to small changes of temperature (rarely exceeding 0.02°C.) and to variations of atmospheric pressure; these effects were very serious in the case of dilute solutions, which could only be examined in cells provided with the newest and most active membranes and in periods of steady barometric pressure. The problem of constructing a manostat for use in these experiments is under consideration.

The most serious disaster in the whole course of the work was an infection of the laboratory with *Penicillium glaucum* during rebuilding operations on a lower floor, which necessitated the constant use of antiseptics during the whole of the subsequent four years. The precautions used suggest the practice of a bacteriological rather than of a chemical laboratory. The two germicides which were most effective in destroying the spores without injuring the membranes were thymol and gaseous prussic acid; all the solutions used in the measurements were sterilised by the addition of thymol to 0.001 normal concentration, saturated solutions being used for storing the cells during the vacation. The mould seems to feed upon the membranes. The first evidence of infection is usually the fact that membranes which were previously rendering satisfactory service show signs of leaking and fail to recover their fully semi-permeable character when resubjected to the membrane-forming process.

5. *The Measurements.*—The main results of the measurements are summarised in Tables I. and II.

The upper part of each table shows the final measurements of the osmotic pressure of cane-sugar solutions between 0° and 25° as carried out in the years preceding 1911. In this range the ratio of osmotic pressure to "gas-pressure" is absolutely steady, so that Gay Lussac's law may be applied rigidly. The observed osmotic pressures exceed, however, the corresponding gas pressures by an amount that ranges from 6 to 11.4 per cent. The ratios in the case of the decinormal solutions rise in a somewhat surprising manner and there is a further remarkable rise when solutions of this concentration are examined at 0° ; as this is within 0.2° of the freezing-point of the solution it is possible that the effect is in some way due to the polymerisation of the solvent.

The lower part of the two tables shows the measurements that have been made during 1911 at temperatures above 25°

TABLE II.

[illegible]

These measurements required the construction of a new series of thermostats and were very costly in other ways on account of the extreme brittleness of the heated glass. Thus whilst the measurements at 30° were begun with an equipment of sixteen manometers, the preparation of which had cost more than a year's labour, not less than twelve of these were put out of commission, half of them permanently, during the course of the work between 60° and 80°.

The results, however, are both striking and important. As soon as 25° is passed the ratio of osmotic pressure to gas pressure begins to drop (in the case of the more dilute solutions with perplexing rapidity), the result being that, in the case of each of the ten concentrations examined, this ratio falls to unity at some temperature below 80° C. In the case of the decinormal solution this equality is maintained over the range from 30° to 60°; in the case of the more concentrated solutions, experiments now in progress will show whether the ratio remains constant at unity or whether it diminishes to some smaller figure.

E. LORD BERKELEY'S EXPERIMENTS

The experiments of the Earl of Berkeley and Mr. E. G. J. Hartley "On the Osmotic Pressures of some Concentrated Aqueous Solutions" are published in the *Philosophical Transactions* for 1906 (A. 208, 481-507). Two additional papers "On the Osmotic Pressures of Aqueous Solutions of Calcium Ferrocyanide" are published in the 1908 and 1909 volumes (*Phil. Trans.*, 1908, A. 209, 177-203; 1909, A. 209, 319-36). Of these three papers the first two dealt with osmotic pressures from 13 to 133 atmospheres, the third covered the region from 15 atmospheres downwards. It will be seen that the work on concentrated solutions takes up and extends to regions of much higher pressure the type of observation that was being made by Morse and Fraser in America. In this region of high pressures a considerable range of substances was examined, including a number of metallic ferrocyanides. Most of the measurements were made at one temperature, 0° C., the object of the experiments being to determine the absolute values of the osmotic pressures and not the temperature coefficients.

* *The Osmotic Apparatus.*—This was of a different type from that used by Pfeffer and by Morse. The chief novelty consisted

in placing the membrane and the solution on the outside of a porous tube instead of on the inside of a porous pot. The apparatus is shown in Fig. 6.

AB is a porcelain tube, 15 cm. long, 2 cm. external diameter and 1.2 cm. internal diameter with glazed ends, CC is a gun-metal cage against the ends of which the dermatine rings DD are compressed when the parts E and F of the outer gun-metal vessel are screwed together, thus making a tight joint with the tube. Another dermatine ring X provides a tight

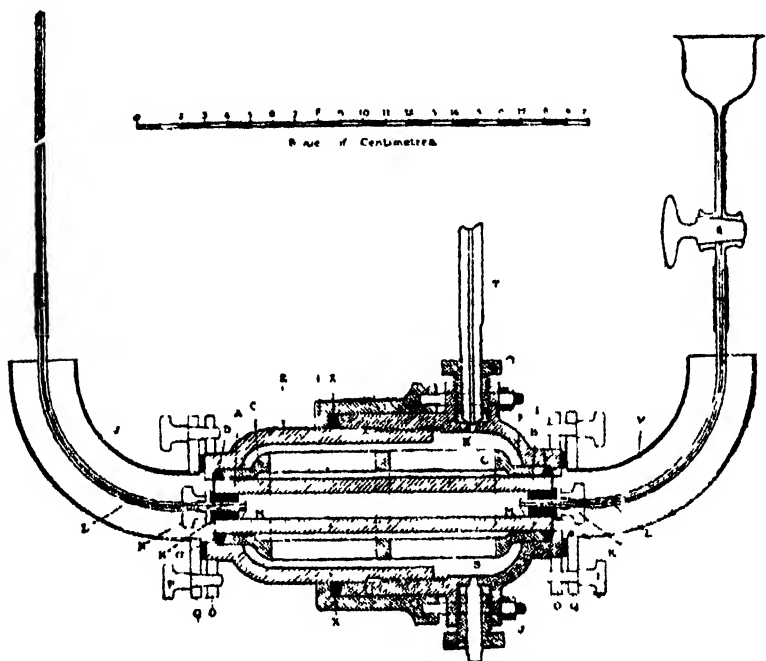


FIG. 6.—Osmotic apparatus complete.

joint between E and F and allows the solution in EF to be compressed without leakage. The water inside the porcelain tube is enclosed between rubber stoppers KK, carrying the brass tubes LL and compressed between the washers MM and the nuts NN. One of these brass tubes carries a glass funnel and tap, the other an open glass capillary or water-gauge, graduated in millimetres and calibrated; this capillary serves to show the rate of flow of water through the membrane, either from the water inside to the solution outside or under high mechanical pressure from the solution to the water. Two

curved metal tubes VV of larger diameter are clamped against the ends of the brass case EF, the joint being made tight by a rubber washer. The case EF is filled and the pressure transmitted through the aperture at R, whilst the aperture at S serves to empty the vessel.

The Pressure Apparatus.—The pressures were measured by means of a dead-weight standard pressure-gauge, fig. 7. This has two plungers, one supporting a series of weights, whilst the other, actuated by a screw and wheel, compresses the "steam cylinder" oil which lies between them. The plunger supporting the weights, which forms the pressure-gauge of the instrument, was kept slowly rotating by hand whenever it was in use. The apparatus can be worked up to 136 atmospheres

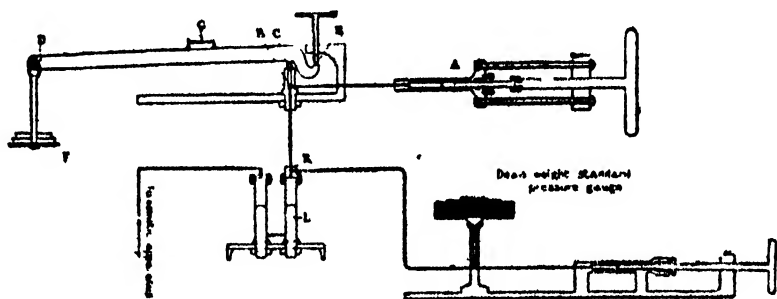


FIG. 7 —Pressure apparatus.

but as the sensibility is about 0.12 atmosphere throughout, the percentage error is increased greatly at low pressures.

The Semi-permeable Membranes.—To deposit the membranes the porcelain tube was placed in a solution of copper sulphate (50 grammes in a litre) in a desiccator and the air exhausted during several days until no more bubbles appeared from the tube; the tube was then removed, wiped inside and out and allowed to dry during three-quarters of an hour. After closing the ends of the tube with rubber plugs carrying glass rods, it was plunged with a spinning motion into a solution of potassium ferrocyanide (42 grammes in a litre), allowed to soak and then set up for electrolysis, the current being passed from a copper electrode immersed in copper sulphate solution inside the tube to a platinum electrode immersed in a ferrocyanide solution outside the tube; the platinum electrode was enclosed in a porous pot to prevent the alkali liberated there from attacking

the membrane and the solution in the pot was frequently changed. When at the end of about two hours, the resistance of the tube had risen to a steady value, the tube was washed and soaked with distilled water, during about ten days, until every trace of copper sulphate had been washed away.

After washing, the loose ferrocyanide was rubbed off with pumice-stone and the membrane remade electrolytically at intervals of a few days until the resistance of the tube rose to some 50,000 ohms. The tube was then tested in the osmotic apparatus with a solution of cane sugar containing 660 grammes in a litre and giving an osmotic pressure of about 100 atmospheres. After washing, remaking and testing several times, steady values for the osmotic pressure were reached; but out of some 100 tubes of various makes which were tried only two reached the highest state of efficiency, although over 400 electrolyses were made. As a great improvement was effected when the membranes were exposed to pressure, a special apparatus was devised in which the electrolytic deposition of the membrane could be carried out under a pressure of 130 atmospheres outside the tube and atmospheric pressure inside. It was also found to be a great advantage to deposit the membranes at 0°C . and to keep them at 0°C . until required for measurements at this temperature.

The Measurements.—Three operations were involved in the measurement of the "equilibrium pressure," *i.e.* the hydrostatic pressure which was required exactly to balance that set up by osmosis.

(a) Guard-ring leak. As the semi-permeable membrane is never quite on the surface of the porcelain tube, it is impossible to get perfect contact between it and the dermatine packing; there is therefore always a leakage of the compressed solution past these guard-rings. This leakage would not matter but for the fact that the hydrostatic pressure on the solution gradually diminishes as it oozes out until it finally escapes under a pressure that is only atmospheric. Minute portions of the membrane are therefore in contact with uncompressed solution and through these water is steadily drawn from the tube into the solution. This effect was reduced by making the guard-rings overlap the ends of the tube and its magnitude was rendered constant by filling the metallic extension-tubes VV with solution so as to provide an ample reservoir of uncom-

pressed solution. The extent of the guard-ring leak was determined by filling the extension-tubes VV with solution whilst the rest of the apparatus was charged with water both inside and outside the membrane. In the final series of experiments the guard-ring leak was reduced, from a rate equivalent to that produced by a pressure of 2 or 3 atmospheres on the solution, to a rate equivalent to only about 0.15 of an atmosphere and therefore almost negligible.

(b) Determination of the turning-point. The chief operation was to determine the pressure at which water just ceased to be drawn from the porcelain-tube into the solution and commenced to flow in the opposite direction. After measuring the guard-ring leak at 0° the space surrounding the porcelain-tube was emptied, rinsed with solution and filled as quickly as possible. The apparatus was then immersed again in ice and pressure gradually applied by increments of about 10 atmospheres until within 10 per cent. of the equilibrium pressure, when smaller increments were applied at longer intervals until the rate of flow was almost exactly equal to the guard-ring leak. During the process of filling and before the pressure was applied a small quantity of water was drawn through the membrane into the solution, giving rise to a film of slightly diluted solution on the surface of the tube; by applying pressure gradually in the manner described the excess of water was driven out again and the solution restored to its original concentration without damaging the membrane. Measurements of the rate of flow with pressures a little above and a little below the turning-point were made at intervals of an hour or more until it was clear that a definite and steady value had been reached.

(c) Solution-leak. At the close of the experiment the apparatus was taken down but in such a way that the porcelain tube and its contents remained intact. After two days the water in the tube was washed out and the sugar-content determined, this process being repeated until no more sugar could be extracted from the interior of the tube. No attempt was made in the later experiments to apply a correction for the greatly reduced leakage of solution, which seemed to have no regular influence on the "turning-point": instead, all experiments were rejected except those in which the leakage of sugar was proved to be less than 0.0003 gramme, a stringent test which eliminated

all measurements except those made with two tubes of pre-eminent excellence.

The Numerical Results.—The figures obtained in the measurements of concentrated solutions were as follows :

					Concentration, G./litre.	Pressure in atmospheres.
Cane sugar	180.1	13.95 12.45 (calc.)
"	300.2	26.77
"	420.3	43.97
"	540.4	67.51
"	660.5	100.78
"	750.6	133.74 51.9 (calc.)
Dextrose	99.8	13.21
"	199.5	29.17
"	319.2	53.19
"	448.6	87.87
"	548.6	121.18
Galactose	250	35.5
"	380	62.8
"	500	95.8
"	(recovered)	.	.	.	500	97.2
Mannitol	100	13.1
"	110	14.6
"	125	16.7

The results are shown graphically in fig. 8, in which the diagonal lines show the values calculated from van't Hoff's equation.

Experiments on Calcium Ferrocyanide.—The experiments on calcium ferrocyanide, published in 1908 and 1909, are noteworthy as extending the measurements of osmotic pressure to aqueous solutions of salts. In the case of the more concentrated solutions the osmotic pressure was correlated with the vapour pressure by means of a thermodynamic formula. In a formula put forward by Prof. A. W. Porter the compressibility of the solution and solvent were taken into account and these quantities were therefore measured but deviations amounting to 2½ per cent. were found between the calculated and observed values. A modified equation was therefore developed in which the thermodynamic cycle was calculated for operations carried out under atmospheric pressure instead of in a vacuum. The deviations were then reduced to less than 0.5 per cent.; but it is noteworthy that the correct assumptions to be made in working out the thermodynamic cycle were only determined

after direct measurements of osmotic pressure and of vapour pressure had been made. Here again then practice has served as a guide to theory and direct measurements have alone proved adequate to justify the validity of the formula in which the thermodynamic relationships find expression.

In the paper on weak solutions of calcium ferrocyanide direct measurements of osmotic pressure were correlated with measurements of electrical conductivity. Once again the conditions

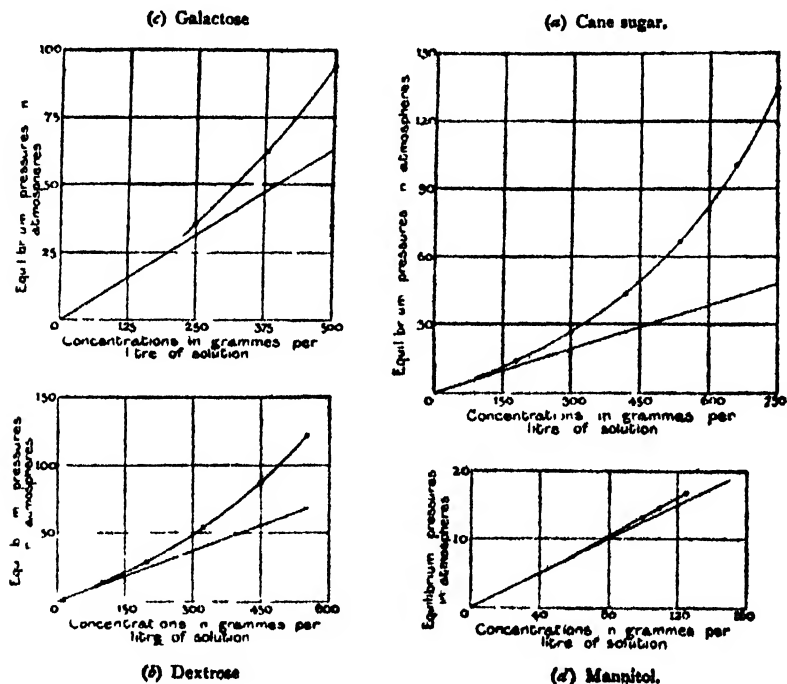


FIG. 8.—Influence of concentration on equilibrium pressure.

were too complex to be expressed by the simple formulæ usually applied to such solutions; but with the help of the new observations it was possible to find suitable assumptions by means of which the experimental results could be expressed and formulated.

F. THEORETICAL CONSIDERATIONS

On comparing the exact measured values of the osmotic pressures, as recorded by Morse and by the Earl of Berkeley, with those calculated from van't Hoff's equation, the latter is

seen to be capable of giving only a very approximate expression of the actual facts. In his latest paper Morse has shown that at temperatures higher than atmospheric each solution in turn reaches a point at which a modified form of van't Hoff's equation gives a correct value for the osmotic pressure but it is not yet clear whether this agreement is only momentary or whether it persists over a large range of higher temperatures. At temperatures from 0° to 25° the deviations recorded by Morse amount to 6 to 12 per cent. even when using the modified form of van't Hoff's equation, whilst Lord Berkeley has recorded at the freezing-point an osmotic pressure nearly three times as great as the values calculated from the equation in its original form.

The various attempts to calculate the osmotic pressures of cane-sugar solutions are summed up by Findlay (*Scientia* 1912) in the following table for 20° C.:

TABLE III

Weight normal concentration.	Volume normal concentration.	Osmotic pressure observed.	Osmotic pressure calculated according to thermodynamic equation.				Error.
			Van't Hoff.	Morse.	Neglecting hydration.	Assuming 6H ₂ O.	
0.1	0.098	2.59	2.34	2.39	2.38	2.40	0.19
0.2	0.192	5.06	4.59	4.78	4.76		
0.3	0.282	7.61	6.74	7.17	7.14	7.40	0.21
0.4	0.369	10.14	8.82	9.56	9.51		
0.5	0.452	12.75	10.81	11.95	11.87	12.54	0.21
0.6	0.532	15.39	12.72	14.34	14.24		
0.7	0.610	18.13	14.58	16.73	16.59	17.93	0.20
0.8	0.684	20.91	16.36	19.12	18.94		
0.9	0.756	23.72	18.08	21.51	21.29	23.52	0.20
1.0	0.825	26.64	19.73	23.90	23.64	26.42	0.22

The concentrations are shown (1) in gramme-molecules per 100 grammes of water and (2) in gramme-molecules per litre. The observed osmotic pressures are shown in the third column, whilst the remaining columns show the values calculated by means of four different formulæ.

Van't Hoff's equation $PV = RT$ may be thrown into the form

$$P = \frac{RT}{V} = \frac{RT}{V_0} \frac{n}{N} = \frac{RT}{V_0} \pi$$

in which V_0 is the molecular volume of the solvent and π is the ratio of the number of gramme-molecules n of the solute to

the number of gramme-molecules N of the solvent in a given volume.

This equation is valid only for very dilute solutions and utterly fails to represent the experimental figures, *e.g.* in the case of the normal solution the observed and calculated figures are in the ratio 3 : 2 approximately.

The thermodynamic equation which expresses the properties of an ideal solution over the whole range of concentration takes the form

$$P = \frac{RT}{V_0} \{ -\log_e(1-x) \}$$

$$= \frac{RT}{V_0} x \left\{ 1 + \frac{1}{2}x + \frac{1}{3}x^2 \dots \right\}$$

This gives the figures shown in the sixth column. These agree quite closely with those calculated by Morse from a modified form of Van't Hoff's equation in which the concentrations are reckoned in gramme-molecules of sugar per 1000 grammes of water instead of per 1000 c.c. of solution. Morse's equation may be written in the form

$$P = \frac{RT}{V_0} \left(\frac{x}{1-x} \right)$$

$$= \frac{RT}{V_0} x \{ 1 + x + x^2 \dots \}$$

The close agreement of the values in columns 5 and 6 is accounted for by the fact that the two equations differ only by $\frac{1}{2}x^2 + \frac{1}{3}x^3 \dots$, quantities that are not important except at very high concentrations.

But neither Morse's equation nor the thermodynamic equation is completely satisfactory, as both are inaccurate to the extent of some 10 per cent. throughout. The thermodynamic equation is based on the assumptions that solvent and solute mix without liberation of heat or change of volume to form an incompressible solution in which the components are present in their normal molecular form, without association, dissociation or combination. Such a description cannot be applied to a solution of cane sugar in water and ample explanations are here forthcoming to account for the breakdown of the thermodynamic formula. Foremost amongst these is the explanation suggested by Morse and Fraser, that the sugar at low temperature probably forms hydrates which break down when the temperature is raised. The figures given in the seventh

column have been calculated on the assumption that the sugar in the solution is present as $C_{12}H_{22}O_{11} \cdot 6H_2O$.¹ A remarkable result is seen on studying the list of errors tabulated in column 8; although the calculated and observed figures are even now not in agreement, the error is quite steady throughout at 0.19, 0.21, 0.21, 0.20, 0.20, 0.22 atmosphere; such a result indicates that a formula has at last been arrived at which expresses the properties of the solutions perfectly, with the exception that some factor, the nature of which is still undisclosed, increases the observed values by one-fifth of an atmosphere above those which have been calculated for the hydrate $C_{12}H_{22}O_{11} \cdot 6H_2O$.

In the above pages, the opinion has been asserted that direct measurements of osmotic pressure are of such vital importance that the enormous labour that has been expended upon them has been both legitimate and fruitful. If the detailed story of these arduous experiments serves to bring home to readers some idea of the motives that inspired the workers and of the difficulties that they had to overcome, the purpose of the writer will have been fully carried out.

¹ The figures given by Findlay are for $5H_2O$; his corrections have been increased in the ratio 6 : 5 to give the figures tabulated in column 7 of the table.

THE COMPARATIVE ANATOMY OF THE INTERNAL EAR IN VERTEBRATES

By R. H. BURNE

EVERY one is familiar with the streak, known as the lateral line, upon the sides of fishes; it can be observed any day upon the fishmonger's slab. But it is perhaps not so universally known, though a matter of common knowledge to any one at all acquainted with comparative anatomy, that this line and similar ones upon the head and face shelter a series of cutaneous sense organs, of simple structure but unfortunately at present of enigmatical function, known collectively as the "organs of the lateral line."

In all probability it is in this system of sense organs of the skin, peculiar to aquatic vertebrates, that we must look for the birthplace of the ear. For in the first place, we have some evidence¹ of a rough similarity in function between the two; in the second, there are certain anatomical peculiarities,² particularly of the nerve supply, that indicate beyond all reasonable question that the ear and the lateral-line organs belong to one and the same sensory system and that the ear is only a lateral-line sense organ specially set apart and so refined as to act, in the first place, as an equilibrating organ for recording alterations in the position of the body; in the second, though possibly only among terrestrial vertebrates, as an auditory organ sensitive to vibrations of the surrounding medium too subtle to be felt by the sense organs of the skin.

In this article an attempt is made to give a general idea of our present knowledge, based upon the work of Retzius,³ of the more important changes of structure that have accompanied this elaboration and refinement of function.

To grasp the significance of the individual steps in the process—for often the changes are in themselves insignificant—

¹ Parker, *Bull. Bureau Fisheries*, 24, 1904, p. 185.

² Beard, *Zool. Ans.* vii. 1884, p. 142; Ayers, *Jour. Morph.* vi. 1892, p. 1.

³ Retzius, *Das Gehörorgan der Wirbeltiere*, Stockholm, 1881-4.

bony labyrinth. In studying this chart-diagram it is in the first place essential to realise that in the membranous labyrinth there are two absolutely distinct structures enclosed one within the other. The inner part known as the *endolymph labyrinth* is the actual sense organ—the seat of the sensory elements in connexion with the filaments of the otic nerve. It is left white in the diagram. The outer part (the *perilymph labyrinth*) forms a sheath to the endolymph labyrinth fitting it tightly or loosely in different parts. In the diagram it is represented as partly opened, its cavity being dotted.

This perilymph sheath is really no part of the sense organ at all but is simply a portion of the mechanism by which vibrations are conducted to the sense organ. The distinction between these two parts cannot be too clearly recognised, for in higher vertebrates and particularly in the cochlea of mammals, parts of the outer sheath are so intimately blended with the enclosed endolymph labyrinth that it is difficult without reference to their past history as revealed by comparative anatomy to realise that they are not integral parts of a single organ.

The endolymph labyrinth apart from its perilymph casing can further be conveniently divided for study into two regions physiologically distinct, the one, which forms practically the entire labyrinth in aquatic vertebrates, being an organ for equilibration, the other (peculiar to terrestrial vertebrates) being specialised for audition. In fig. 2 these regions are respectively represented by the parts of the labyrinth known in man as *vestibular*—that is the semi-circular canals, utricle, saccule and (in lower vertebrates) the lagena—on the one hand; and the cochlear canal or pars basilaris lagenæ (the *scala media cochleæ* of human anatomy), on the other.

In tracing the evolution of the vestibular or equilibrating part of the labyrinth, it will be unnecessary to consider the perilymph sheath, for this only comes into prominence in terrestrial vertebrates as an accessory to the auditory organ.

In every endolymph labyrinth, except only those of the Lampreys and Hag-fishes, there are certain constant features subject of course to minor variation but always recognisable. Three semi-circular canals surmount and open into the saccular chambers that form usually the bulk of the labyrinth. Each canal has always at one end a swelling (fig. 2, AMP.) crossed

transversely by an upstanding ridge of sensory epithelium—the canals and sensory ridges being so set that each lies approximately in one of the three planes of space.

Upon the walls of the saccular chambers are three sensory areas (fig. 2, dotted areas in Rec. utr., Sacculus and Lagena) each covered by an otolith or mass of calcareous matter and stated to lie, like the sense organs of the semicircular canals, approximately in the three planes of space.

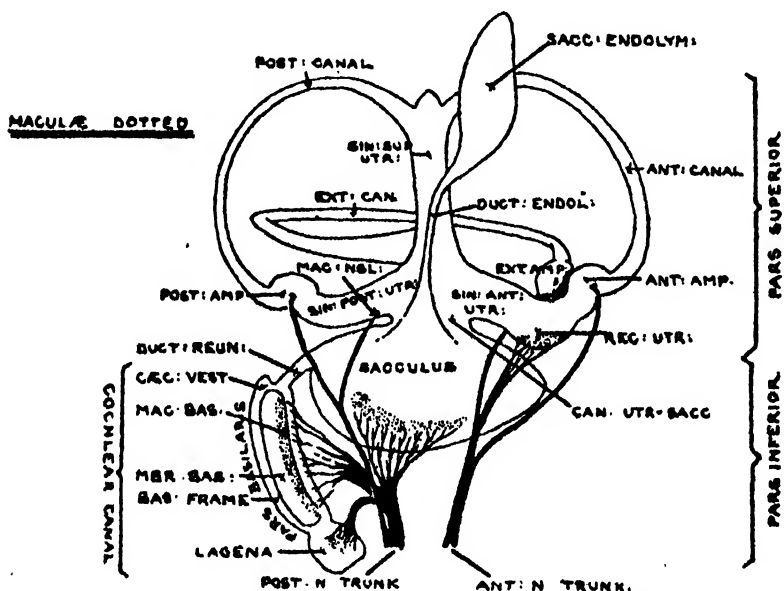


FIG. 2.—A schematic left endolymph labyrinth seen from the mesial aspect, showing all the chief structures ever found in this organ.

The nerve endings are dotted. The whole labyrinth, except the pars basilaris, constitutes the equilibrating labyrinth (vestibular of man). The recessus utriculi, sacculus and lagena contain the three otolith organs. The sense organ of the pars basilaris (*mac. bas.*) constitutes the organ of Corti.

There are thus in the typical vestibular labyrinth two sets of three sense organs so arranged that the members of each set are aligned with some sort of accuracy in the three planes of space—an arrangement approximating to that theoretically the best for response to movements in any direction.

And as a matter of fact there is a mass of experimental evidence from 1828 onwards¹ to show that these sense organs are concerned primarily in response to changes in the position

¹ Flourens, *Mém. Ac. R. Sci. Inst. France*, t. 9, 1828, p. 455.

of the body—the sense organs in the canals being probably stimulated by the impingement against them of the fluid in the canals displaced by rotational movements of the head, the otolith organs being, in a similar way, stimulated by the drag or pressure of the otoliths upon them during movements in direct lines or through alteration in the resting position of the body.¹

In land vertebrates, as we shall see later, an additional sense organ arises between the sacculus and its lagenar appendage (fig. 2, Mac. bas.) and undergoes progressive elaboration to form in conjunction with parts of the perilymph system a special auditory organ independent of the vestibular parts of the labyrinth.

Such an organ as the above typical endolymph labyrinth, even before the advent of the cochlea, is obviously very far removed from a simple lateral-line sense organ but the relationship between the two can be traced in the early development of the ear.

At its first appearance² the ear, like many other epidermal sense organs, is a little superficial thickening. Further growth transforms this into a pit, which sinks deeper and deeper into the mesodermal tissues, becoming a long-necked flask, like one of the isolated lateral-line organs of the skin, with a single area of modified epithelium to represent the sense organ. This may be considered to represent the lateral-line stage of the ear.

In most cases the flask now becomes nipped off from the surface and begins to develop characteristics peculiar to the ear.

The first indication of anything distinctive is the formation of a narrow fold along the upper border of the vesicle.³ This is the budding canal system and heralds the formation of the two canals that lie in the vertical planes. The horizontal canal in almost all cases appears later. This sequence in the canal formation is particularly interesting, for in the few cases (Hag-fishes and Lampreys) where there are only two canals, it is the horizontal canal that is missing.⁴

Variation in the form and relative length and width of the semi-circular canals is decidedly capricious⁵; frequently a canal

¹ Lee, *Jour. Physiol.* 15, 1894, p. 311, and 17, 1894-5, p. 192.

² Krause, *Handbuch der Entwicklungslehre*, L. 4 and 5, 1893.

³ Krause, *Arch. Mikr. Anat.* Bd. 35, 1890, p. 287; Fleissig, *Anat. Hfte.* 71, 1908, p. 69.

⁴ Tretjakoff, *Anat. Anz.* 32, 1908, p. 165.

⁵ Wulf, *Arch. f. Anat.* 1901, p. 57.

does not even lie in the same plane throughout its length but takes a sinuous course between one end and the other. The facts, so far as we have them, seem to suggest that the course, length and width of a canal are not of vital physiological importance, provided that the sensory ridges and the stretch of the canals leading to them are accurately aligned at right angles to one another and in the three planes of space. The rest of the canal, if approximately in the same plane, serves its purpose by facilitating the flow of the endolymph across the sensory ridge when the head rotates.

In the lowest fishes—the semi-parasitic Hags—the saccular chamber into which the two ends of the combined vertical canals open is single and has a single sensory area covered by a single mass of calcareous material.

But in all other fishes most if not all of the chambers and sense organs normal to the vestibular labyrinth are recognisable. Interesting stages in the separation of the different parts may be observed in many Sharks and other fishes, especially in the Lamprey,¹ the general tendency being towards a more complete isolation of the different sense organs. In the Wolf-fish and some other teleosts, this tendency may, in fact, be carried to such an extreme that the sacculus and lagena are completely cut off and lie more than half an inch away from the rest of the labyrinth.

Above the lowest fishes, all parts of the vestibular labyrinth can be traced either in adult or embryonic life throughout the whole Vertebrate Class, although in some cases one part, in some another, may suffer degeneration. In all, however, there are the three semi-circular canals lying in approximately the same relative positions; and in all, except in mammals other than the monotremes, there are three sensory areas covered by calcareous material.

In comparing the whole labyrinth of a fish with that of man, for instance, it is plain that although in the fish all parts of the human labyrinth except the cochlea are represented, they are represented in excess, being vastly larger and more complete. The vestibular or equilibrating labyrinth in man and all higher vertebrates is, in fact, to a certain extent degenerated. This fact requires some explanation if it indicate a diminution of efficiency, for it entails no apparent loss of balancing power,

¹ Krause, *Anal. Anz.* 20, 1906, p. 257.

One can only suppose, as has been suggested by some physiologists, that it is a more or less direct result of the greater share taken in equilibration among higher vertebrates by sense organs, other than the ear, of improved efficiency and power of co-ordination.

On the other hand, when we consider the fact that in fishes there are only those parts of the ear present to which, by common consent, powers of equilibration alone are ascribed, we are confronted by the interesting question whether it is to be expected or rather whether there is any evidence to show that fishes have any true sense of hearing, seeing that in their ear there is no structure at all comparable to that by which this function is performed in terrestrial vertebrates.

This is a question that has exercised the minds of naturalists since very early days. It was one of the problems that engaged John Hunter¹ in the eighteenth century but it appeared then far more simple of solution than now, for it was taken for granted that, if fish were sensitive to noises, the labyrinth, from its resemblance to the human ear, without question must be the organ affected; further, no distinction was drawn between coarse mechanical vibrations that can be felt and true molecular sound vibrations that can only be heard.

Hunter attempted to solve the problem as presented to him by a very simple experiment and was quite satisfied with the result.

While serving with the army in Portugal, he chanced to be watching a pond in which Gold-fish were swimming. To test their sensitiveness to sound, he got a friend who was with him to fire a gun screened from the fish by some bushes. No sooner was the gun fired than the fishes vanished into the mud at the bottom of the pond.

Now this and similar experiments show that fish are sensitive to shock or jar but that is all. They give no clue to their power of true hearing nor as to whether the labyrinth is the organ affected and if so what part of it is the actual receptive organ.

Since Hunter's day, experiments have been carried out with the object of answering these questions but so far with perplexing and inconclusive results.

A few abstracts from some recent work on the subject will show the position.

¹ Hunter, *Phil. Trans.* 72, 1782, p. 379.

In 1895 Kreidl¹ made some experiments upon Gold-fish from which he concluded that the fish ear was not sensitive to sound or indeed to any vibration but that coarse vibrations were felt by the skin.

He tested the fishes by means of vibrating rods plunged into the water of the tank and with instruments of various sorts sounded in the air. None of these vibrations elicited the least response but the slightest jar to the water was responded to at once—the response being quite independent of the presence or absence of the ear but dependent on the full physiological activity of the skin.

Similar results as to the total insensitiveness to musical tones were obtained in 1907 by Lafite Dupont² and Körner.³ Various kinds of fishes were tested with tuning-forks and instruments specially constructed not to produce tangible vibrations.

Thus it would seem to have been fairly settled that fishes could not hear in the true sense of the word

On the other hand Parker⁴ and subsequently Bigelow⁵ obtained results from Minnows and Gold-fish the precise opposite of those got by Kreidl. They found that the fish responded to the vibrations of a tuning-fork when the ear was intact but that when the ear was rendered inactive by cutting the otic nerve all response ceased in spite of the fact that the skin remained in full working order.

This is a surprising want of harmony in results obtained by similar experiments upon the same species of fish. It can perhaps be explained, as suggested by Bigelow, by the practical difficulties that bar the removal of the whole ear by the method of extraction used by Kreidl. And if, as seems likely, the lower saccular chambers (sacculus and lagena) were left behind in his experiments, his conclusion that the ear has no part in vibration perception is vitiated.

All these experiments were performed on fish in captivity and therefore in a somewhat abnormal state; but, in 1903, Zenneck⁶ carried out some very careful experiments upon fish

¹ Kreidl, *Arch. f. Physiol.* 61, 1895, p. 450.

² Lafite Dupont, *C.R. Soc. Biol. Paris*, 63, 1907, p. 710.

³ Körner, *Arch. hydrobiol. Stuttgart*, 2, 1906, p. 9.

⁴ Parker, *Bull. U.S. Fish Commission*, 22, 1902, p. 45.

⁵ Bigelow, *Am. Nat.* 38, 1904, p. 275.

⁶ Zenneck, *Arch. Physiol.* 95, 1903, p. 346.

living their ordinary natural life. The essential points of his experiments were (1) the use of fish in their natural environments; (2) the use of a powerful source for the sound (a bell some 17 cm. in diameter); (3) great care in shielding the water in which the fish were swimming from heavy mechanical vibrations set up by the bell; (4) the location of the source of sound in the water.

The results of his experiments showed that the fish were sensitive to the sound of the bell within a radius of some 8 to 10 yards.

Anyhow, after Parker had succeeded in satisfying himself that the ear was sensitive to vibration, in 1908 he proceeded to try to locate the actual receptive organ.¹ Taking the Squeteague, a fish in which the sacculus is of very great size, he attempted to put the great saccular sense-organ out of action by pinning the otolith away from the sensory epithelium. Under these conditions nearly all response to vibration was lost. Parker therefore concluded that the otolith organs were the seat of a vibration sense.

Further confirmation that the otolith organs respond to sound is furnished by an important experiment by Piper.² When the otoliths are exposed in the severed head of a Pike and brought within range of the sound of a pipe, electrical changes occur in the otic nerve such as are normally associated with the passage of a nervous stimulus.

In addition to the above direct experimental evidence of a generalised and dull auditory power in fishes, there is evidence of an indirect circumstantial character that also points to the same conclusion.

In the first place, many fishes,³ particularly among the Sciaenidæ, Siluridæ and Triglidæ, make sounds which are quite distinctive and sometimes remarkably loud. Possibly, in some cases, these sounds are the by-products of some other activity; they may also be accompanied by mechanical vibrations that can be felt. Whether the fish are also sensitive to the true sound vibrations, it is almost impossible to say; the fact that they make them, often as a secondary sexual action, favours the assumption that they are also sensitive to them.

¹ Parker, *Bull. Bureau Fisheries U.S.A.* 28, 1908.

² Piper, *Munch. med. Wochenschr.* 53, 1906, p. 1785.

³ Tower, *Annals N.Y. Acad. Sci.* xviii, 1908, p. 149.

Then there are also those peculiar and intricate connexions between the swim-bladder and the ear that are to be found in Carps, Siluroids, Herrings and a few other bony fish. These certainly, by their structure, suggest an organ for transference of vibrations. Though it is of course held by many, including some, like the late Prof. T. W. Bridge, who have made a very special study of these connexions, that they are hydrostatic and serve to inform the fish of the condition of tension in its swim-bladder and therefore of its depth in the water, it should be borne in mind that they occur mainly in bottom freshwater fishes who can have comparative little opportunity for alterations, in the depth at which they swim so great as to be a vital matter. Unsatisfactory as the present condition of the question of hearing in fishes undoubtedly is, the general trend of the above evidence suggests that fishes are sensitive, through the ear, to shock and jarring vibrations of any sort and are also to some extent capable of hearing true sounds if the sounds are sufficiently loud and are originated in the water. The actual receptive organs for vibration are probably the otolith organs.

We must now consider certain modifications that arise in connexion with the equilibrating ear that ultimately lead to our own organ of hearing—the cochlea and organ of Corti.

The first appearance of these modifications coincides with the adoption of a terrestrial mode of life, which is quite what one would expect, seeing that in air sound plays an infinitely more important part in life than can be the case in the relatively profound silences of the sea. It is not, therefore, matter for any surprise that the organism responds to its new conditions and attempts to form an organ more sensitive and accessible to sound vibrations than is the deep-seated labyrinth of the fish.

This object has been attained by modifications in three directions:

(1) By the formation of a direct path by which vibrations may reach the capsule within which the ear lies (the tympanic apparatus).

(2) By the provision of efficient means for directing the vibrations after they have entered the ear capsule to certain definite nerve endings.

(3) By an elaboration of the nerve endings themselves.

Dealing with the second of these lines of modification,

it will be found that invariably the process has been of a similar character.

Provision is always made by means of an open and definite perilymph cavity for a direct and unimpeded passage for vibrations between an opening in the outer skull wall (*fenestra ovalis*) and a similar opening elsewhere in the wall of the otic capsule. This perilymph passage at a certain definite spot or spots is separated from the cavity of the endolymph labyrinth by tense drum-like thinnings of the labyrinth walls and near or on these thinnings there is a nerve-ending which becomes very highly specialised in the higher though simple in the lower groups.

Thus in the simplest and most direct way provision is made for unimpeded movements of the fluid around the endolymph labyrinth and for the transference of these movements from the perilymph to the endolymph at certain definite spots.

In the labyrinth of a fish, except for a thickening beneath the sensory areas, the walls of each particular region are of fairly uniform thickness or at least there is no sudden change from thick to thin.

In the amphibia this is not so.¹ Among them, except in the lowest purely aquatic Urodeles, certain restricted areas of the endolymph labyrinth wall are thinned down to the lining epithelium, whilst around them the walls are suddenly thickened like a frame.

These framed thinnings in the wall of the endolymph labyrinth are the first sign of an auditory organ.

In amphibia where they first appear, there are three of them which almost might be spoken of as tentative experiments in the manufacture of an auditory instrument, for one of them only has apparently stood the test of experience, the one namely that is situated in a special dilatation between the saccule and the lagena.

This dilatation (*pars basilaris lagenæ*), with its thin area, stretched like a drum-head in its frame, at its first appearance is inconspicuous enough but though so insignificant for the moment, it is potentially of the very highest importance, for it is from this paltry rudiment that the human cochlea with its intricate powers of hearing has been evolved.

There is at first sight little in the structure of the *pars*

¹ Harrison, *Internat. Monthly Jour. Anat.* 19, 1902, p. 224.

basilaris lagenæ of the amphibian to suggest the great coiled cochlea that dominates the labyrinth of the mammal. But close inspection in the light of a knowledge of this part of the ear in reptiles and birds leaves no doubt that even when it first appears, this *pars basilaris* has in it in rudiment some of the most essential peculiarities of the cochlea.

In man and other mammals the cochlea, as every one knows, consists throughout almost its entire length of three fluid-filled channels—a central one (*scala media*) wedged in between two others (*scala vestibuli* and *S. tympani*) (fig. 1). The central channel is a direct process of the endolymph labyrinth. It is triangular in cross section, with the apex directed to the axis of the cochlear spire, its base applied to the surrounding bony envelope, one side (morphologically the outer) covered by the *scala vestibuli* and the other (morphologically the mesial) covered by the *scala tympani*. This third side consists partly of a bony shelf (*lamina spiralis*) projecting from the axis of the spire and partly of a thin membrane (*membrana basilaris*) tensely stretched throughout the whole length of the cochlea between the edge of the bony shelf and a corresponding fibrous ridge (*ligamentum spirale*) projecting from the wall spoken of above as the base of the triangle. Covering the axial half of the basilar membrane is a strip of sensory epithelium of peculiarly intricate structure, known as the *organ of Corti*.

These, from the point of view of comparative anatomy, are the essential characters of the *scala media* or cochlear canal.

The two other *scalæ* (*S. vestibuli* and *tympani*) are in open communication at the apex of the cochlea. The *scala vestibuli* is a continuation of the general perilymph space that lies between the oval window and the vestibular parts of the endolymph labyrinth. Just short of the tip of the *scala media*, it passes into the *scala tympani*, which follows the *scala media* to its base and there terminates in contact with the membrane-covered round window. Near the round window the *scala tympani* is connected with the brain cavity by a narrow tube (*canalis perilymphaticus*). (Fig. 1, Aqued. peril.)

The fluid in these two continuous perilymph *scalæ* is thus in a position to respond readily to every swing of the stapes in the oval window and to transmit its movements to the sense organ in the *scala media*. In fact these perilymph *scalæ* are parts of the mechanism for the transmission of vibrations to the

sense organ quite accessory to the sense organ itself. For our present purpose the essential things to note are (1) the tense but thin basilar membrane stretched from end to end of the scala media, in a rigid frame (figs. 1, 2, Mbr. bas.); (2) the close relations of the basilar membrane to the brain cavity and the exterior through the mediation of a definite perilymph space (*scala tympani*).

These two characters are in fact the only ones to suggest that the pars basilaris of the Amphibia is a cochlea in the making. There is in this group of vertebrates no scala vestibuli and no organ of Corti but there is a thin circular basilar membrane framed in a cartilaginous thickening of the surrounding walls, and applied to the exposed (*i.e.* mesial) surface of this basilar membrane is a little perilymph sac (*scala tympani*) (fig. 3, amphibian, P. bas., Sc. tymp.) which is in close connexion with the brain cavity and with the exterior on the one hand and on the other by means of a tortuous but definite tube (fig. 3, D. PLPH) with a great vestibular perilymph space (Sp. sacc. fig. 3) lying between the fenestra ovalis (fig. 3, f. ov) and the sacculus. At present there is no prolongation of this vestibular perilymph chamber upon the outer surface of the pars basilaris—no suggestion in fact of a scala vestibuli. The sense organ of the pars basilaris at present lies near but not upon the basilar membrane.

In this primitive condition of the auditory organ, there are none of those refined peculiarities of structure that we are accustomed to associate in the cochlea of higher vertebrates with a power to analyse compound musical notes. There is no specialisation of the sense organ such as we see in the organ of Corti, no fibred structure of the basilar membrane and no regular variation in size and number of the various elements of which the different parts are composed. It is thus very doubtful whether we should be justified in regarding these modified tympanal areas in the endolymph labyrinth of the amphibia, with their associated perilymph chambers, as anything more than mechanisms for focussing vibrations upon certain sensory areas.

But although we can scarcely credit amphibia, on structural grounds, with a musical sense, there is every reason to suppose that differences in the rapidity or complexity of the vibrations, beating upon the sense organs in the ear produce recognisable

differences in the character of the stimulations transmitted to the brain.

Apart from any question of sound analysis, it is well recognised that frogs have a very shrewd power of discrimination,¹ as they respond with the greatest alacrity to the croaking of their own species, whilst to other sounds even of the most varied and alarming or seductive description they may remain to all appearances deaf.

Among reptiles the cochlea makes great strides towards perfection. In the lowest forms it is scarcely present at all, in crocodiles it is practically the same as in a bird. In all, however, even the lowest, there is one very significant change: the sense organ of the pars basilaris lies *on* the basilar membrane. This is a difference that marks a distinct step towards the perfection of the cochlea and possibly means the initiation of an entirely new mode of stimulation. In any case, whatever the precise physiological meaning, it is one of the distinctive anatomical characters of the organ of Corti as opposed to an ordinary sense organ of the labyrinth that it should rest actually upon a thin, tense and probably vibratile membrane in the direct path of vibrations passing across the scala media, not upon the surrounding thick and stationary wall of the labyrinth.

In the further evolution of the cochlea, two tendencies may be observed—one leading towards an increase in the length and complexity of the scala media, particularly as concerns the basilar membrane and the sense organ, the other making for greater simplicity² of the perilymph spaces. The tendency to elaboration results in an increase in the number and a regular variation in the size of the sensory elements and of the various structures associated with them; the tendency to simplification of the perilymph spaces ensures that the sense organ is suspended in the direct and unimpeded path of movements originating at the fenestra ovalis.

In reptiles the cochlear canal or pars basilaris lagenæ can be found in any condition between that of a lowly amphibian and that of a bird. In different genera of snakes and lizards it and its sense organ show a progressive increase in length culminating in the tubular and slightly twisted cochlea of the

¹ Courtis, *Am. Nat.* 41, 1907, p. 677; Yerkes, *Jour. Comp. Neurol.* 15, 1905, p. 279.

² Gray, *Proc. Roy. Soc.* 80, 1908, p. 507.

crocodile, with its long basilar membrane stretched in a correspondingly elongated cartilaginous frame.

In all the reptiles, with the possible exception of the crocodiles, the changes in the structure of the cochlea are apparently quantitative rather than qualitative. The sense cells still have the diffuse arrangement of those of an otolith organ. They show no regularity in disposition or variations in size, nor are they supported in any peculiar manner. In fact the sense organ has as yet assumed none of the special features of the organ of Corti.

It is curious how bird-like the cochlea of the crocodile is. It stands quite apart from that of other reptiles and shows many peculiarities of structure, insignificant in themselves but of the greatest interest as the shadowy rudiments of important structures still to come. Thus, the basilar membrane is not only long but differs in width in different parts and contains a layer of stretched diagonal fibres; the elements in the sense organ show a distinct tendency towards orderly linear arrangement and a structural differentiation amongst themselves; the membrane floating above the sense organ (*tectorial membrane*) is now for the first time anchored along one edge to the supporting frame of the basilar membrane, stretching out hood-like over the surface of the sense organ.

All these slight changes are worthy of the closest attention for they are in embryo characters peculiar to the cochlea in its more perfect developments and indicate the rise among the higher reptiles of an auditory organ not simply sensitive to sound but probably to some extent capable of resolving complex tones into their components and thus of judging the *musical quality* of sound. Here in fact for the first time, in the crocodiles and birds, we meet with an auditory organ of something the same kind as our own.

Although the cochlea in mammals is always unmistakably mammalian, in the monotremes it has not yet shaken off all traces of the reptile. While these traces are just in process of elimination we may digress for a moment to reconsider and complete their history.

The first is a small sense organ to which we have not hitherto alluded. It is known as the *macula neglecta* (fig. 2, Mac. ngl.) and was discovered by Retzius in many fishes. Although present in most fishes, it reaches the height of its importance in

amphibia, where it is related to one of the experimental auditory organs mentioned above. In reptiles and birds it again sinks into insignificance; in monotremes and possibly other mammals¹ it appears for a moment in the embryo; in the adult it has gone.

Another interesting organ that disappears (at least functionally) in the mammals is the *lagena*. This chamber with its otolith organ is first separated off from the sacculus among the sharks. It is a conspicuous object in the labyrinth of bony fish. In amphibia and most reptiles it still holds its own against the encroachment of the growing pars basilaris which intervenes between it and the sacculus. In birds it has become a mere terminal appendage of the now preponderant pars basilaris. It is still present as a sense organ in adult monotremes but in other mammals it persists merely as the non-nervous tip of the cochlear canal—a functionless vestige.

Other reptilian characters may be recognised in peculiarities of the perilymph scalæ and will be referred to again later.

Stripped of these surviving relics, the mammalian cochlea very closely resembles that of man. Differences occur in the length of the cochlear canal and in its mode of coiling² but in all essentials there is great uniformity and this is nowhere more apparent than in the detailed structure of the sense organ—the organ of Corti.³

This organ, which is absolutely characteristic of the ear of mammals, has an extremely elaborate and definite construction into which it is needless to enter now. It must suffice to emphasise certain essential peculiarities.

The cells that compose this sense organ have an absolutely regular disposition. The sensory hair cells are set in parallel rows from end to end of the cochlea.

All the elements—the sensory hair cells, the supporting cells, the “Pillars of Corti”—increase regularly in both number and size from the base of the cochlea to the apex. A similar increase is noticeable in the size of the tectorial membrane that floats above the sense organ and in the breadth of the basilar membrane and therefore in the length of the transverse cords of which it is composed.

¹ Alexander, *Jena Denkschr.*, Bd. VI. Th. 2, 1904; Stutz, *Morph. Jahrb.*, 44, 1912.

² Gray, *The Labyrinth of Mammals*, vol. i, 1907, 22.

³ Kolmer, *Arch. mikr. Anat.*, 70, 1907, p. 695, and 74, 1909, p. 259.

These peculiarities are probably extremely important parts of the mechanism by which complex tones are resolved into their components and are essential to the due performance of the higher functions of hearing. But before entering further into this question we must return and study for a moment the evolution of the perilymph spaces connected with the cochlea—the scala vestibuli and tympani.

As mentioned above, the history of the perilymph spaces is essentially one of simplification, as pointed out by Dr. Gray. When we left these spaces in the amphibia (fig. 3, amphibian), there was only a little rudiment of the scala tympani pressed against the mesial surface of the basilar membrane but as yet no signs of a scala vestibuli on the outer surface of the pars basilaris. The scala tympani was nothing but a slight protrusion from the side of a tube (fig. 3, D. PLPH) that connects the great perilymph chamber lying between the saccule and the oval window (fig. 3, Sp. sacc.) with the cranial cavity and the exterior. In reptiles the arrangement is essentially the same (fig. 3, reptilian) except for the advent of a scala vestibuli (Sc. vest.), which is represented by a downward prolongation of the saccular perilymph chamber upon the outer surface of the pars basilaris or cochlear canal. The two scalæ, although present, are not connected directly through their apices but indirectly through the perilymph duct and the saccular perilymph chamber.

In crocodiles, so far as our information goes, in birds, certainly, there is a direct connexion, though an imperfect one, by means of a loose spongework of tissue that surrounds the apex of the cochlear canal (fig. 3, Bird) and is in open connexion with the cavities of both scalæ. As soon as this direct connexion appears the indirect connexion through the perilymph duct is lost. Finally in monotremes a free passage (fig. 3, monotreme, HLCTR) is opened up between the apex of the scala vestibuli and the apex of the scala tympani and the two scalæ become a continuous tube running down the outer surface of the cochlear canal from the vestibular perilymph space (foramen ovale) and up the mesial surface to the foramen rotundum.

The loss of the perilymph duct in crocodiles and birds, however, is not complete. A considerable part, somewhat swollen, remains between the scala tympani and the cranial cavity and the exterior (foramen rotundum), forming a definite *perilymph sac* (fig. 3, SAC. PLPH). Very pronounced traces of

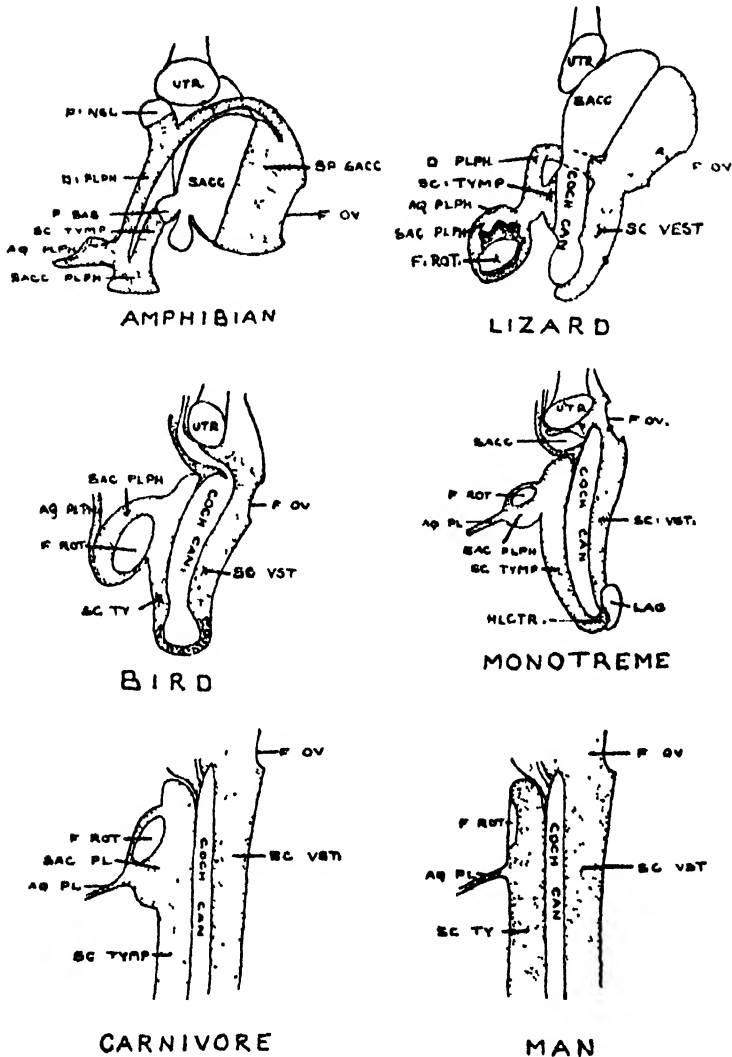


FIG. 3.—Four diagrams, based mainly on Mr. Harrison's and Dr. Gray's papers, illustrating the transformation of the perilymph spaces in Amphibia, Reptiles, Birds, and Mammals.

The perilymph spaces are dotted, the endolymph labyrinth white. In each case the ear is the left in section, seen from behind. The scala media of the cochlea is marked *P. bas* (in amphibian), *Coch. can.* in the rest; the saccular chamber, SP. SACC.; the connexion with the brain cavity, AQ. PLPH, or AQ. PL. The open connexion between the two perilymph scales in Monotreme is marked HLCTR.

this sac are present in adult monotremes and it may still be recognised in many of the lower mammals, sinking gradually

more and more into the general body of the scala tympani, till at last the connexions with the brain cavity (*canalis perilymphaticus*) and the exterior (*foramen rotundum*) become sessile upon the wall of the scala tympani itself.¹

So from the very simplest beginnings, by gradual elaboration of the sense organ and simplification of the path by which vibrations may reach it, our ear has reached its present form. It is, however, one thing to pick a complex piece of mechanism to pieces, quite another to explain its working. And that is just the present position. The structure of the ear is fairly well known, its action is still very obscure. At present there are two classes of theory by which it is sought to explain the mechanism of hearing: by one (the telephone theory) the vibrations transmitted to the cochlea are supposed to act upon the sense organ as a whole and the resolution of complex sound is referred to the brain, by the other (the resonance theory) the preliminary sorting is done by the ear. By the various resonance theories, amongst which that of Helmholtz still holds the field, the analysis of complex sounds is supposed to depend on the sympathetic vibration of some part of the cochlea to each particular note and the selective stimulation of corresponding sensory cells of the organ of Corti.

Such theories rest upon the ordered distribution and regular increase in length, size and number of the various elements of the cochlea to which reference was recently made, which is such a striking and remarkable feature in the anatomy of this organ.

The parts most frequently regarded as the resonators are the parallel cords lying in the basilar membrane upon which the sense organ (the organ of Corti) rests, like piano-wires stretched between the lamina spiralis and ligamentum spirale (fig. 1, Membr. bas.). Those cords that by their length and degree of tension are in tune with any particular note vibrate in unison with that note and tap the sensory hairs of the sense cells resting upon them against the lower surface of the tectorial membrane that floats like a hood above them.

This is the theory.

Recently it has been questioned seriously whether the basilar membrane and organ of Corti are by their structure capable of acting as they should do upon this theory and

¹ Gray, *Proc. R. Soc.* 1908, p. 521.

certainly a formidable array of difficulties can be raised on the anatomical side.

It has, for instance, been said that the basilar membrane in all its parts is too thick and too narrow¹ to be set in sympathetic vibration by sound, although as a matter of fact a model of the basilar membrane, an indiarubber sheet 0.5 mm. broad, has been made to vibrate in sympathy with a tuning fork. There seems, therefore, to be no physical reason why the basilar membrane should not be thrown into sympathetic vibrations but recent histological research² raises doubts whether the fibres of the membrane are sufficiently free to vibrate independently. Instead of lying more or less isolated and free in a homogeneous semi-fluid bed, they are now shown to be bundles of fibrous tissue loosely felted together at all points and thus quite incapable of the individual movement generally assumed to be necessary to satisfy the demands of the Helmholtz theory.

But supposing the fibres are capable of sufficient individual movement, it is maintained that their vibration would immediately be damped by the soft tissues that cover both surfaces³ of the basilar membrane.

Yet further objections may be urged with regard to the number of the cords.

For the theory to hold good, it is necessary that there should be fibres in sufficient quantity and of sufficient variation in length to resonate to every distinguishable note. Now we can fairly gauge the hearing limits of certain birds by their powers of mimicry. The parrot⁴ in particular has obviously an extremely critical and discriminative ear with great appreciation of the quality of sound. But in its cochlea there are only some 1,200 cords in the basilar membrane⁵ with little or no variation in length except towards the extreme base. Here undoubtedly is a very formidable difficulty to the Helmholtz theory, at least among birds.

Supposing, however, that the basilar membrane in mammals is capable of doing all that is required of it under the theory, it

¹ Shambaugh, *Am. Jour. Anat.* 7, 1907, p. 247.

² Hardesty, *Am. Jour. Anat.* 8, 1908, p. 156.

³ Kishi, *Arch. ges. Physiol.* 116, 1907, p. 121.

⁴ Denker, *Biol. Col.* 26, 1906, p. 600.

⁵ In man there are some 24,000.

has been shown recently that many of the elements that compose the organ of Corti are more firmly united than was supposed, too firmly to be capable of any individual movement¹; finally it has been pointed out that in the pig parts of the sense organ, apparently fully formed and functional, rest upon bone and not upon the basilar membrane at all.²

All explanations of the working of the cochlea are so purely a matter of speculation that it is necessarily difficult to prove whether this or that objection is fatal to the Helmholtz or any other theory.

Of course if a physicist can show that a membrane of the size and thickness of the basilar membrane cannot possibly vibrate in sympathy to musical tones there is an end of the matter so far as it is concerned.

But short of this, the other objections just mentioned, although matters for serious consideration, do not seem to be necessarily fatal.

By a modification of the Helmholtz theory, Dr. Gray³ shows very conclusively that for the basilar membrane to act as a resonant analyser, it is not by any means necessary that single or even small groups of cords should alone vibrate for each perceptible note.

On the contrary every note would produce sympathetic vibration in a more or less extensive area of the basilar membrane; but in this area the part most accurately in tune with the particular note would be in maximum vibration and would give to the whole stimulation the colour of that particular note.

Although we may say, I think, that the Helmholtz theory or some variant of it still holds the field as the orthodox explanation of the action of the cochlea, an alternative resonance theory, based on the structure of the tectorial membrane, has recently been revived.

Prompted by the structural difficulties to the Helmholtz theory that have just been mentioned, certain anatomists⁴ in Japan and America insist that the tectorial membrane by its

¹ Hardesty, *Am. Jour. Anat.* 8, 1908, p. 157.

² Shambaugh, *Am. Jour. Anat.* 7, 1907, p. 247.

³ Gray, *Jour. Anat. and Physiol.* 34, 1900, p. 324.

⁴ Kishi, *Arch. ges. Physiol.* 118, 1907, p. 112; Shambaugh, *Am. Jour. Anat.* 7, 1907, p. 245; Hardesty, *Am. Jour. Anat.* 8, 1908, p. 109.

position, variation in size, fibrillar structure, low specific gravity and extreme flexibility, is better fitted than the basilar membrane to respond to every vibration of the endolymph and to be set in motion in its different parts in sympathy with notes of different rapidity and they maintain that it, not the basilar membrane, is the active agent in the stimulation of the sense-cells of the organ of Corti. The sense-cells do not strike the tectorial membrane but the tectorial membrane strikes the sense-cells.

The actual mode of stimulation of the auditory organ must for the present remain undecided. There is, however, one and that a fundamental question upon which it is possible to speak with more certainty. There is evidence, both clinical and experimental, to show that the cochlea is in itself, in some way, a mechanical analyser of sound. For it is certainly affected in different parts by notes of different pitch.

In cases of partial deafness (deafness to particular notes) it has been shown by post-mortem examination that particular parts only of the organ of Corti are destroyed.¹ When the deafness is to notes of high pitch it is the basal parts where the elements of the cochlea are at their smallest and shortest, when to notes of low pitch, the apical.

Similar results have recently been obtained by direct experiments upon guinea pigs.² Guinea pigs kept during long periods under the influence of one note were found to have part of the organ of Corti destroyed. The higher the note the nearer the base of the cochlea was the spot.

One can therefore conclude with some degree of safety that the cochlea is the organ by which complex sound vibrations are mechanically sorted and analysed. The perception and appreciation of the results of this analysis depend of course upon the brain. The ear can only furnish the brain with the raw material of assorted stimulations; it depends upon the brain by its innate powers and by practice to realise and appreciate the shades of difference there are between these stimulations.

¹ Bezold, *Zeits. f. Psych. u. Phys. d. Sinnesorgan*, 1896, xiii ; Gruber, *Allg. Wien. Med. Ztg.* 1864, ix.

² Yoshii, *Zeits. f. Ohrenheilk.* 58-59, 1909, p. 240.

THE PROJECTED REVIVAL OF THE FLAX INDUSTRY IN ENGLAND

By J. VARGAS EYRE, PH D.

FLAX at the present time is worth nearly twice as much as it was some eight or ten years ago and there seems to be little chance of a return to the former level of prices. Apparently, the increased cost of the raw fibre is due entirely to the operation of natural economic conditions and cannot be attributed to commercial manipulation. It is therefore not surprising that attention is being directed to the question of the practicability of reviving the flax industry in this country. More particularly is this the case in view of the desire to encourage a return to agricultural pursuits and to increase the number of small holdings, flax being a crop which is better suited to the conditions under which a small holder of land is placed than to those of the farmer of a large acreage. Flax is a good alternative crop and for this reason alone would be useful as an addition to the usual rotation; moreover, as weather which suits flax grown as a fibre crop is not good for corn, in a season in which cereals fail flax will probably succeed.

Judging from past experience it may be said that when the difference between the price of wheat and the price of flax is large, then the latter becomes a profitable crop in this country. At the present time such conditions obtain. It is noticeable also that the linen trade of Europe is dependent upon the supply of middle and low quality fibre coming from Russia and that the industrial and agricultural development of this country is exercising a marked influence on the price of flax and tends to keep the prices high for the following reasons. Whilst the area under flax is not increasing, the Russian linen industry is developing rapidly: already practically the whole of the best quality fibre grown in that country is absorbed within the Russian Empire. The agricultural development of Russia and the opening up of new areas to wheat in Western Siberia and Asiatic Turkey have the effect

of reducing the profit attending wheat growing in other countries and both these circumstances operate to make the chance of successfully reviving the flax industry in our country more favourable.

~ The possibility of successfully reviving the industry has been seriously considered by the Development Commissioners; indeed, the revival of both flax and hemp industries was specifically mentioned in the Act of Parliament which brought that advisory body into existence. During the past two years much first-hand information has been gathered by studying the subject of flax cultivation and fibre separation in the chief flax-growing countries of Europe, namely Russia, Holland, Belgium, France, Ireland, Austria-Hungary and Germany and the information has been presented in the form of a Report. Moreover, certain field experiments were conducted last year in Bedfordshire, where, besides raising the crop, retting experiments were made in tanks especially constructed for the purpose.

The result of the inquiry made on behalf of the Development Commissioners leaves no room for doubt that the climate of this country is well suited to flax. The crop makes no special demand for a particular class of soil, so long as the land is properly prepared and suitably manured. Light loam, however, may be said to be most favourable and chalk least favourable, to a fibre crop. Large areas of suitable land are to be found in Yorkshire and Somersetshire, as well as in the midland and eastern counties. Flax can be grown successfully as a fibre crop in this country and at the same time the seed which it bears can be profitably saved; indeed, this is the practice which was formerly adopted. The flax crop is somewhat more troublesome than the usual farm crops but no difficulty in its cultivation need be apprehended provided practical information be placed at the disposal of farmers. This could be done easily and there is every reason to believe that good crops of flax would again be raised here if attention were given to the work.

The somewhat complicated and troublesome operation of separating the fibre is not considered to fall properly within the province of the agriculturist. The labour at his disposal is unskilled for the most part and he is able to give only divided attention to the preparation of the fibre, whereas skilful

handling and careful watching are necessary if good results are to be achieved. The preparation of uniform fibre of good quality should be the object in view, if the revival of the flax industry is to be successful, because labour in this country is too costly for low quality home-grown fibre to compete successfully with that which is imported from Eastern Europe, where the labour of preparation is disregarded when reckoning the cost of production.

The possibility of cultivating and separating the fibre at a profit cannot readily be decided ; there are many contingencies which are difficult to evaluate and much that is hypothetical enters into the problem. The general evidence obtained is undoubtedly favourable ; indeed, the opinion was expressed in the Report to the Commissioners that practical trials on a moderate commercial scale can alone afford the definite knowledge that is required as to the degree of financial success that will attend the production of flax fibre in this country. The possibilities opened up, if the scheme proved successful, are held to be ample justification for its serious trial. In this connexion it is very noteworthy that the English flax industry existed longest in those districts where there was a central retting depôt to which the harvested crop was carried and sold by farmers and, at the present time, there is very reasonable foundation for the belief that on these lines the flax industry could be successfully revived.

Strong reason was found for the belief that the judicious revival of the flax industry, managed according to improved methods, would be productive of benefit to British agriculture and would afford people an opportunity of finding regular employment in rural districts by creating a demand for skilled labour.

It has been recommended that one or more small retting depôts be established out of public funds in suitable localities—for instance, in Yorkshire and in Somerset—each capable of dealing with the produce of about one hundred acres. Such establishments, managed on strictly business lines during a few years and conducted as experimental stations, would enable the required information to be gained as to whether the cost of the after-processes of preparing the fibre can be brought sufficiently low to make the flax crop once more a profitable one to the farmers of Great Britain. This is necessary because, although the re-establishment of flax as a

farm crop is the main object in view, it becomes necessary to find a market for the straw and this involves organising the after-treatment of the crop, namely the retting and cleaning. It is with these operations that the chief difficulty is encountered.

The Commissioners have now had the Report on the management of the flax industry before them and they have received the recommendations contained therein favourably. With the object of carrying out, in this country, the necessary practical trials above mentioned, a society has been formed under strict conditions of *non-profit trading*, in order that it may be eligible for a grant from the Development Commissioners, who are expressly empowered by the Act of Parliament which established the Development Fund to encourage the cultivation and preparation of flax and hemp in Great Britain.

In view of the interest which has been aroused already by this line of action, the Commissioners have kindly given their consent to the publication of the following résumé of the Report referred to.

HISTORICAL

Somewhat extensive flax growing and fibre production in England is still within the memory of many people in certain rural parts of the country; but, at the present day, there is little to indicate the extent of this lost industry. The names of such places as Flaxton (Yorkshire), Little Steeping (Lincolnshire), Retford (Notts) and Flax-Bourton (Somerset) seem to be some of the best evidence for locating the scene of flax cultivation in the past. Separation of the fibre from the straw was formerly part of the agricultural practice in England just as it is in Russia at the present day, the cleansing and preparation of the fibre providing work during the winter months for the husbandman and his family.

Flax growing in England probably dates from the Roman occupation, although practically no mention of it is to be found in official records until A.D. 1175, when flax was included among titheable articles, from which fact it is concluded that the cultivation of the crop had attained to considerable dimensions at that time. In 1532, an Act of Parliament was passed which compelled all persons holding tillage land to sow at least one rood with flax for every sixty acres of such land occupied. After thirty years, this law was made more stringent, a penalty

of £5 being imposed upon persons not growing at least one acre of flax for every sixty acres of land cultivated.

With the object of still further encouraging the growth of flax in England, the tithe on this commodity was reduced to 4s. per acre in 1691 and in 1712 a bounty of one penny per ell was given on all exported British-made sail cloth. In 1806 a bounty was offered for the importation of flax from British Colonies and every effort was made to increase the production of fibre at home so as to supply the requirements of the growing British industry more completely.

At that time flax was grown more or less in every part of England and in many counties several thousand acres were annually under this crop; but the supply of raw material did not keep pace with the home demand, as may be seen from the Parliamentary Returns of that period, in which fairly large imports of flax are recorded.

Flax suffered considerable depreciation on the introduction of cotton and the success obtained in spinning cotton fibre by machinery led to a further reduction in the demand for linen, as it was impossible for that material to compete with the low price of cotton fabrics. About 1820 steam-driven flax-spinning machinery became commercially successful and the demand for flax fibre became greater in consequence; but, at that time, the difference in the value of a flax crop and of a wheat crop was insufficient to induce the better farmers of this country to embark again on the troublesome task of preparing the fibre. British flax culture fell into discredit, apparently owing to the fact that only low quality fibre was prepared and whilst the quantity grown in England diminished, the amount imported became steadily larger. To take one county as an example, in 1810 between 4,000 and 5,000 acres of flax were grown in Dorset but in 1850 the acreage under the crop had fallen to some 300 acres.

Writing in the *Journal of the Royal Agricultural Society of England* in 1847, J. MacAdam states that the great markets for flax supplying the spinning trade were Leeds, Belfast and Dundee; the finest yarns were made by English spinners, the great bulk of medium yarns by Irish manufacturers, Scotland producing the very coarsest. MacAdam advocated the more extensive cultivation of flax in the United Kingdom and showed clearly that a profit of £10 per acre was obtainable at that time provided cultivation were carried on in the proper manner.

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There was a revival of English flax-growing about 1850 but development was arrested by the greatly enhanced price of corn, so that for the time being flax was outclassed as a farm crop. Furthermore, following the Treaty of Paris in 1856 and peace with Russia, very large quantities of cheap Russian fibre came to British markets; and this seems to have been the blow from which English flax production has never properly recovered, although various attempts have been made to restart the industry.

The custom of working large farms and the increased value of produce requiring less attention and less skilled labour occasioned a decline in the area devoted to flax and a marked disinclination on the part of the agriculturist to do more than grow the crop and harvest it. The establishment at this time of depôts at which the straw was received and worked up into fibre mark a new stage in the history of English flax.

The adoption of the system of centralising the after-processes led to a revival of the industry about 1860, when considerable quantities of flax were grown: in fact, in 1870 the area devoted to flax in Great Britain was 23,957 acres, the greatest area occupied by the crop in any year on record. About 1875 a succession of bad seasons was experienced in England; this circumstance and the keen competition of foreign flax fibre and Manilla hemp, as well as the high price of wheat, caused many farmers to cease growing flax and soon afterwards several works were closed down. In 1876 flax works were established at Long Melford (Suffolk) and continued working during about twenty years; several smaller attempts were made to revive the industry in Suffolk prior to 1888 but without success. To judge from the quantity of straw dealt with annually, the most prosperous mills were those at Selby and Staddlethorp in Yorkshire. At the former, the crop from nearly 2,000 acres was handled successfully but the quantity raised fell off considerably, until in 1896 not more than a 500-acre crop was dealt with at Selby and the mills at Staddlethorp had the crop from barely 200 acres. It is, however, significant that both the mills surviving in 1896 were conducted as central retteries and that the principle of retting in tanks of warmed water had been adopted. Since that time, flax has been grown as a fibre crop only to a very small extent: small areas have been seen from time to time both in Yorkshire and in Somerset; in the latter

county there is some grown still, which is dew-retted and sold locally.

AGRICULTURAL REQUIREMENTS

There is considerable diversity of opinion expressed as to the particular soil which is best suited for the production of flax as a fibre crop. It is frequently stated that a well-drained loam gives the best results and rich loamy clays are considered to be very suitable. Whilst on the one hand it is maintained that good flax can only be raised on good rich soil, it is not infrequently asserted that the nature of the soil is of small importance. From a general examination of the soil in the principal European flax-growing areas the writer has formed the opinion that there is much truth in all these statements: apparently good flax can be raised on a great variety of soils provided their texture be suitable. Very heavy clay is not favourable for flax, neither is chalk and there is good evidence for saying that soil which is very rich in humus is unfavourable, also peaty moorland; but almost any other "clean" land which is capable of producing good crops of grain will produce good crops of flax.

The flax plant grows very rapidly, sending down a fine filamentous root system as far beneath the surface of the soil as the stem rises above it. The subsoil therefore must be of a kind which will allow of root development to the full extent and at the same time be sufficiently compact to offer a firm hold for the plant: in fact, conditions which are most favourable to the growth of wheat. It is of great importance to the production of good uniform fibre that the plant should develop at a steady rate and receive no check during growth—indeed, these conditions are of paramount importance when flax is grown for high quality fibre. Although rich land will produce what appears to be a splendid crop of healthy tall plants, when they are examined they are found to yield an amount of fibre not at all in proportion to the luxuriance of growth and at the same time to be of a lower value for spinning purposes. Although stress is frequently laid upon the advisability of sowing flax on rich soil, on strong deep loam, it is a singular circumstance that most of the good flax grown is produced on very light soil, often on sand.

Generally speaking, it may be said that in Ireland the best

flax comes from a gravel soil with gravel subsoil: in the north of France excellent flax is grown on a very light sandy loam and the soil of East Flanders is very similar to the French, although it differs from it in containing a larger proportion of sand and in being in a better condition owing to the high cultivation that has so long prevailed in Belgium. The flax soil of West Flanders is somewhat heavier than that of East Flanders, as it contains a larger proportion of clay and in some cases approaches the composition of the heavier marlish-loam known in Holland as "*Zeeklei*." This is a deposit of sand rich in clay which is widely distributed: it "weathers" readily, forming a good porous firm soil and it may be said that flax cultivation in Holland is confined to the regions of that particular deposit.

The flax districts of Russia are so extensive that it is difficult to formulate a general statement as to the class of soil yielding the best crops. It may be said, however, the chief characteristic is lightness, the soil being composed largely of sand. The poor, sandy, scrub-land between Vologda and Tver produces flax of excellent quality and when it is properly farmed and sown remarkably good crops are raised. This type of soil extends eastward as far as Viatka and Perm and the whole region is a flax-growing area; but in the western provinces of Pskoff, Vitbesk, Livonia, Kurland and Kovno the soil is somewhat heavier in consequence of the widely distributed moraine matter in those regions.

Although flax is not a specially delicate crop to grow there are several points in regard to its cultivation which require unusual attention. One of the main factors which make for success is the care with which the soil is prepared for the seed. The importance of cultivating the land to a high degree of firmness is to be emphasised, for therein lies much of the secret of success. Not only must the soil be fine but it must be firmly bedded. It would be difficult to lay too great stress upon the fact that the seed-bed must be deeply worked and firm, with a shallow surface layer of fine soil to cover the seed.

Although flax has long been specially cultivated for the fibre it bears, it is only comparatively recently that attempts have been made to evolve a system of manuring the crop so as to harvest better fibre. The growing period of flax is short; it is only on the land about twelve to fourteen weeks and probably

for that reason it requires its nutritive materials to be in such a form that they are easily assimilable; which means that the application of manure can be made profitably only after a thorough knowledge of the land has been acquired.

Flax is said to be a potash-feeding plant, requiring a good supply of this soil constituent together with lime. Certainly it does appear that this crop grows better on the new "Polder" land in Holland than it does on the old, there being more lime and potash in the soil recently reclaimed from the sea.

The place which flax is most suited to occupy in the scheme of crop rotation is of course dependent upon the soil, upon what is the most marketable produce and upon other varying circumstances. It is certainly an unwise practice to grow flax frequently on the same land, because a condition of soil sickness, known as "flax-sickness," sets in. Where the soil is rather heavy, it is sometimes made to carry two or more crops between a dung manure and a flax crop: for instance, in Friesland the land is well dunged for potatoes and the next year sugar-beet is brought on by artificial manures; in the third year oats are grown with artificial manures; in the fourth, a suitable dressing of artificial manure is given for a flax crop.

A very general practice in all countries is to sow flax after oats or at any rate after some crop which will leave the land as far as possible free from weeds. When the soil is poor in nitrogen, the last oat crop is sown with clover and a clover crop taken before flax is sown; but where the soil is not deficient in nitrogen, leguminous crops are kept well removed from flax and a crop of chicory is taken between oats and flax. Many people in Russia and Holland hold the opinion very strongly that it is best to grow flax on land which has been two or three years under grass.

It is probable that the conditions under which flax is grown at the present time are not at all natural to the plant: the production of tall, straight stems, with little seed and much fibre, having been brought about by long cultivation under particular conditions. The object of the flax-grower is to produce long, uniform, slender stems carrying as much fibre as possible and as little woody material as is compatible with proper stem rigidity.

The actual growing period of flax extends over only about

ten weeks and of this time the early stages are the most critical. When once started the plant grows rapidly, especially during the month of June, when an increase of $1\frac{1}{2}$ to $1\frac{1}{2}$ in. occurs during a period of twenty-four hours. Unless the soil is able to retain a good supply of moisture or frequent light rain falls, this rapid growth receives a check and this causes the fibre to become coarse and irregular instead of increasing in length.

Quite a cool, temperate climate is best suited for the production of a good fibre crop. It is noticeable how generally flax-growing areas are situated near the sea coast, where the crop benefits by the moist wind and the generally uniform climate. Flax is grown extensively in Normandy, Brittany and Picardy, in France; in the northern part of Ireland; over an area extending about 50 miles inland from the Belgian coast; in Zealand and the islands of South Holland, as well as along the coast of Friesland and Groningen in North Holland; and extensively in the Baltic Provinces of Russia. All these districts enjoy similar climatic conditions during the growing period—namely, a rather low, even temperature, rather high humidity and nearly equal rainfall.

Fibre grown in cool, moist regions is fine, silky and possesses good spinning quality; that produced in a district where the summer is hot and dry is short, harsh and dry. This influence of climate on the quality of the fibre was markedly shown in the French and Belgian crops of 1910 and 1911: the former year being wet and the latter unusually dry. Generally speaking, the fibre from the 1910 crop was long, firm, silky and moist, whilst the fibre from the 1911 crop was shorter, stronger and somewhat harsh and dry. It may be said that 1910 gave a weft flax and 1911 a warp flax.

It has been stated frequently that flax is an exhausting crop for the land. All crops are exhausting, but in this case it is intended to imply that *flax removes more from the land than do other crops*. This opinion dates from very early times: flax being stigmatised as a hurtful and exhausting crop by Greek and Roman writers. At the present day, this belief finds expression in some land agreements, wherein the tenant is specifically prohibited from growing flax or is forbidden to remove both the seed and the straw from the farm. Although this belief has been contradicted from time to time, the evidence refuting it has not received due credence because the fact

remains that flax crops cannot be successfully grown at as frequent intervals as other crops. In the light of the experimental work of Snyder, Wolff, Hodge, Tretiakov and others, there can be no doubt that flax removes, if not less, at any rate not more, nutritive materials from the soil than other farm crops.

In this connexion, the work of Prof. Snyder is of particular interest and it is from his results that the following table has been compiled for the purpose of showing the comparative draft of various crops upon the soil :

Average crop in bushels.	Crop.	Pounds of					
		N.	Phosp acid	Potash	Lime.	Silica	Ash.
30	Wheat	52	30	52	12	174	315
40	Barley	40	20	38	9	72	216
50	Oats	50	18	45	11	75	205
30 tons	Mangels	225	105	450	90	30	1050
300	Potatoes	80	52	150	50	8	250
20	Flax	72	24	36	21	47	116

Among the points of interest which are brought out by this table is the fact that a mangel crop is not a good crop to precede flax because of the large withdrawal of nitrogen and potash it occasions—substances upon which flax largely depends for its rapid growth. It is evident also that a crop of flax is no more exhausting to the soil than is an ordinary grain crop.

Other evidence contrary to the view that flax is a particularly exhausting crop has been furnished from the North Dakota Experimental Station, where it has been demonstrated that better crops of wheat¹ can be raised after flax than after wheat. When writing upon this subject Prof. Bolling cites the confirmatory work carried out at Poltava by Prof. Tretiakov, showing the draft on the soil to be less for flax than for wheat, even when water evaporation is taken into consideration.

CHOICE OF SEED

A number of forms of flax are cultivated at the present day which exhibit differences sufficiently well marked for them to be classified by some authorities into varieties of several species. Flax, however, responds so markedly to a change

¹ It is not clear whether due allowance was made for the weeds which presumably were left with the wheat crop and removed from the flax crop.

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of climate or soil conditions that in some of these cases it is difficult to regard the differences observed in the habit of the plant as being due to conditions other than those of growth or environment. The more important varieties which are grown for fibre are:

- | | | | | | | |
|----|------------------------------------|-------------------------|---------------|--------------|---|--------------|
| 1. | <i>Linum usitatissimum vulgare</i> | . | . | . | . | blue flower. |
| | " | " | " | <i>album</i> | . | white " |
| | " | " | <i>regale</i> | . | . | blue " |
| 2. | " | <i>americanum album</i> | . | . | . | white " |
| 3. | " | <i>hyemale romanum</i> | . | . | . | blue " |

Some of these forms are undoubtedly better suited to certain soils than are others; for instance, on the heavier land of Friesland the coarser-growing white flowering flax (*L. usit. var. album*) is exclusively grown, whereas on the adjacent new "Polder" land the blue flax (*L. usit. vulgare*) is found to be more successful; but in other regions, where white flowering flax was formerly grown it has been found more profitable now to grow the blue flowering variety. It is noteworthy that Riga white flowering flax is less liable to disease and gives a heavier return of fibre than Riga blue flowering flax, although its quality, more especially in fineness, is not equal to that of the latter.

Although in all other European countries emphasis is laid upon the necessity of frequently changing flax seed, the country from which the best flax seed is obtained—Russia—knows no such necessity. In Russia, it is generally accepted that the best seed for fibre production comes from the Baltic provinces and the province of Pskoff and Vologda; when occasion arises Russian growers obtain seed from these districts for their own use. The best fibre and the best flax seed are exported from the provinces mentioned and the crops are almost invariably grown from seed of the previous harvest, seed change not being an agricultural consideration. In many cases the farmers have had their seed in the family more than twenty years and although at the present day the yields of fibre are smaller than formerly, there is no such deterioration as is said to take place in Holland and Belgium after growing from the same seed successively during only four or five years. Generally speaking, Russian seed undoubtedly gives a more uniform and more healthy crop than any other, notwithstanding the fact that, owing to increased railway facilities, the time has now passed

when it was possible to say that reputed Pskoff seed came from that province or that Riga seed came from the Baltic provinces.

It is a very noteworthy and general practice in the best flax areas of Russia to dry the seed finally in an oven at a comparatively high temperature. Besides ensuring thorough drying, this operation may possibly act beneficially in killing off imperfectly developed and poor seeds, so that only those of a uniform and high vitality remain. Certainly the process of oven drying is beneficial, apart from the fact that it prevents subsequent heating of the seed when in barrels during transit and may account for the fact that Russian seed gives better crops although the percentage of dead seeds is higher than in any other. Not only has oven-heating been found advantageous to the subsequent flax crop but if the seed be submitted to several degrees of frost a similar result is observed; it is no uncommon practice for Russian peasants to expose their seed to the action of frost with the object of improving the flax harvest raised therefrom.

As already mentioned, the general practice is to rely upon Russia for the supply of flax seed to all countries, the imported seed coming chiefly from the Baltic Provinces by way of Riga. It is then grown in other countries for about three seasons, giving rise to crops bearing seed which is known respectively as "Riga-Child" and "Riga-Grandchild." Where the climate is moist and dull, "original" Russian seed gives the best results; especially is this the case if the soil be light. Where the prevailing atmospheric conditions are dry or the soil is somewhat heavy, better results are obtained by using "Child" seed although the crops raised therefrom are less uniform than those from Russian seed. In Belgium, the best practice is to procure "Dutch-Riga-Child" from some trustworthy source: the particular seed known to come from a good crop of fibre flax grown in Holland the preceding year being the most highly prized. Seed in Holland is ripened naturally in the field better than in other countries and large quantities of "Dutch-Riga-Child" are sown in Holland, Belgium, Ireland and France, where, in many cases, it is sought after in preference to Russian "original" seed.¹

¹ Possibly this may be explained partly by the interesting and quite general observation that whereas Russian "original" seed produces crops richer in fibre, the "Child" seed shows its superiority in producing crops bearing fibre which is finer and of better quality.

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It is possible for those who collect Dutch-grown seed for export to ascertain what the seed has done in the past and to collect only the best for distribution to flax-growers; and as not more than 10 per cent. of the Dutch crop is grown from seed other than that freshly imported from Riga, one can be fairly certain that the seed is Dutch-Riga-Child when offered in Holland under that name. In Russia, this is not yet possible; seed merchants have mostly to buy in small quantities from agents or middlemen who collect smaller quantities from peasant farmers. The Russian merchant has therefore to deal with a great variety of types and is only able to grade his seed according to general appearance, colour, shape, size, etc. and to take care that "Steppe" seed does not enter into his mixtures. By long experience merchants have found that seed from a region where there are certain conditions of climate is better suited for exportation to one country than to another; for example, seed from a very wet district does better in the drier climate of Holland than in Ireland, whilst seed from a drier region is better suited to the damp climate of the north of Ireland. This kind of practical information stands the export merchants in good stead and the accuracy of their judgment is quite remarkable.

SOWING AND AFTER-CULTIVATION

In some quarters it is said to be an advantageous practice to defer sowing flax seed until as late in the season as possible, so as to allow the land to be cleaned of weed seedlings. However true this may be in the case of certain lands where weeds are plentiful, it must be questioned first whether flax is a suitable crop in such cases; moreover the advantages of this practice are far outweighed by those attending early sowing. The best advice is to sow as early as possible, as early as the soil and weather will permit, so that the seed may germinate slowly and have a good start while moisture is in the top soil.

Usually it is possible to sow on light soils at the commencement of April, whereas the end of April is generally sufficiently early for the heavier land such as occurs in Friesland but varying influences have to be taken into account and only the farmer can properly say when his land is in suitable condition. The seed bed must be of fine tilth and it is best to sow on a harrowed rather than on a rolled surface.

For the production of a tall uniform flax crop it is necessary to sow the seed somewhat thickly and although errors may be made in the direction of sowing either too sparingly or too freely, the fault is more often seen of sowing too thinly. This is the worse error because it allows the plants to take on a broader growth and to branch lower down the stem than would be the case were they closer together. Thin sowing brings about an increased yield of seed but the fibre, for which the crop is grown, suffers in being coarser and shorter. The thicker the crop is sown the taller will be the plants before branching, consequently the yield of fibre will be greater and it will be of a finer quality; but of course there are limits to this beyond which it is foolish to go.

Some of the highest rates of sowing in Ireland are from $1\frac{1}{4}$ to 2 bushels per statute acre; whereas in Holland and Belgium as much as 3 bushels per statute acre are used. On the very light soil in North Belgium 2 bushels of seed, 80 per cent. germinating, are sown to the statute acre; on the loam soil in France $2\frac{1}{2}$ bushels and on the new Polder land of Groningen as much as 3 bushels per statute acre.¹

For the most part sowing is done by hand; especially is this the case in Ireland and Russia. It requires exceptionally calm weather and great skill on the part of the sower to obtain anything like an even distribution of the small, slippery seed. A small portable distributing machine known as the *Violin* (or *Fiddle* in England) is extensively used in Holland, Belgium and also in Ireland. The machine is so called because of the to-and-fro motion of a bow-like handle necessary to actuate a distributing wheel which is fitted at the base of the small reservoir containing the seed. This simple little machine is carried under the left arm of the sower and is steadily worked with the right hand as it is carried at a uniform pace over the field. Much of the difficulty attending broadcast sowing has been overcome by its use.

¹ Before sowing it is advisable to test the germination of the seed, because in some cases this varies rather widely. For example, Russian seed of which only 75 to 80 per cent. germinates will not go so far as Dutch seed of which 95 per cent. germinates and this is approximately the extent to which differences are found. Such tests, however, afford no criterion as to the value of the seed for growing good crops and it must be remembered also that they are made under conditions which are very different from those met with in the field, so that much importance should not be attached to the results.

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Few farmers show any inclination to drill the seed by ordinary machines or by any modification of them, although this method of sowing, besides ensuring even distribution, also has the advantage of bedding the seed at a uniform depth. This is a very important thing to achieve with flax because the object is to raise a crop of great uniformity and when the seed is deposited at varying depths irregular germination follows and an irregular crop is the result. Flax must not be laid deeply in the soil; about half an inch is quite sufficient. After sowing, the field is lightly harrowed crosswise and finally rolled lightly so as to consolidate the surface, in order to bring moisture into close contact with the seed and at the same time make the surface of the field flat.

WEEDS AND DISEASES

Well-farmed land is tolerably free from weeds and it is possible by suitably cultivating during the previous season to reduce weeds to a minimum. It must be observed, however, that the nature of the conditions of flax cultivation and the growth of the plant itself seem to be favourable to the growth of weeds. In Holland and Belgium weeding is carefully and thoroughly done by women and children, who go barefooted about the field; kneeling to weed, they go systematically through the field twice and sometimes three times during the months of May and June. Although the wage paid for this class of labour is small (1s. to 1s. 6d. per day of about twelve hours), the cost of weeding in these countries when outside labour has to be procured adds greatly to the cost of producing the crop. Generally, however, the small farmers in those countries have families sufficiently large to enable them to provide most of the labour required for this purpose from their own household.

This necessity for repeated hand-weeding is not recognised in France nor in Ireland; the farmers in those countries are content to remove convolvulus and weeds which make a large and bulky growth, such as thistles, dock and charlock. Excellent flax crops are to be seen in Ireland and also in the north of France, where some of the finest quality straw is raised and taken to Belgium to be retted. The impression produced is that the necessity for close hand-weeding as practised in Holland and Belgium is somewhat over-estimated.

Of the several diseases and pests which affect flax only quite a few make themselves sufficiently prominent to call for mention here: nor need they in any way cause the farmer anxiety. At an early stage of growth, when the plants are only about two inches above the ground, they are sometimes affected by a fungoid disease known as "yellowing" which is stated to be due to the fungus *Asterocystis radialis*.

At a later stage of growth *Flax Wilt* is sometimes manifest; it is a disease attributed to the joint activity of several micro-organisms of which the most definitely identified is *Fusarium lini*. This disease is hardly ever met with in Russia, although it has long been known in Holland, Belgium and France. *Flax Rust* (*Melampsora lini*) may become a serious trouble in some localities where the wild purge-flax (*Linum catharticum*) flourishes, this particular plant being somewhat commonly affected by the disease.

Flax is subject to the ravages of several animal pests but fortunately it suffers to no greater degree than do other farm crops from similar causes. The grub of the *silver Y-moth* (*Pusia gamma*) feeds upon the flax blossoms and the larva of the two flies *Thrips linaria* Uzel and *Haltica nemorum* and also the flax-flea-beetle (*Longitarsus ater* Fab.) may do considerable damage to the young plants.

It has been found in Ireland that a certain local condition of soil occasions a sparsity of some of the plant's requisites, causing small areas of young flax to become yellow and of sickly condition. Rain showers frequently revive such flax but when no rain falls a light dressing of muriate of potash has the effect of restoring the flax to a healthy condition. Save in exceptional cases, it is not customary to apply top dressings to flax but should a spell of dry weather retard the early growth of the crop it is well to apply a light dressing of nitrate of soda; but it must be used with moderation and is only to be given with the object of preventing the crop from receiving an early check to its development. When once the flax crop has made a good start it requires no more attention until about harvest time.

HARVESTING

Only when the crop is grown expressly for seed is it allowed to become quite ripe before being harvested. When grown for the fibre it bears, the matter of harvesting seed is either entirely

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neglected or it is only regarded as of secondary importance. It undoubtedly detracts much from the value of the fibre if flax straw be allowed to remain standing until the seed is ripe; the fibre thereby loses much in spinning quality, becoming dry and inclined to brittleness, besides ultimately weighing less. The cause of these differences is ascribed tentatively to the seed depriving the plant of its oily sap for its own full development.

There are, however, but few districts where the seed borne by the plant is entirely sacrificed. Sometimes this is done in Belgium, where small quantities of flax are harvested almost as soon as the crop comes into bloom with the specific purpose of obtaining fibre of the very finest kind and of the greatest possible elasticity and silkiness for the manufacture of fine lace. Apart from such isolated instances it appears that Ireland is the only flax-growing country where the asset the seed affords is entirely disregarded.

To grow flax primarily for fibre and secondarily for seed is certainly the most advantageous course to pursue and it behoves the farmer to harvest his flax crop at a stage when the seed is developed to the minimum extent for it to be of practical value, in order that the fibre may suffer as little as possible. It is everywhere agreed to be the best practice to harvest flax when the lower part of the stem begins to change from green to yellow—when about one-third of the stem has so changed and when the leaves about half-way up the stem have changed colour or fallen. At that stage, an examination of the seeds within the capsule shows them to be just changing from a full green colour to a brownish tint. These are the general signs that the crop has matured sufficiently and harvest operations should commence at once. Efforts are made to get up the crop as near the same stage of ripeness as possible; no delay is allowable, because during warm summer weather ripening processes proceed rapidly.

When judging of the best method of harvesting flax it is necessary to have in mind the fact that its value is greatly reduced if the straws are not arranged parallel with one another in a neat, uniform bundle—conditions which reduce the waste occurring during the process of cleaning. The advantage of these ideal conditions of harvesting therefore has to be balanced against the cost of attaining to them.

The universal method of harvesting flax is to pull it from the

ground by hand labour. This is due to the fact that no satisfactory machine has yet been devised for pulling it and it is strenuously maintained to be a bad practice to cut it. Why exactly this ban should be put upon cutting is not easy to understand, because an examination of the root end of flax straw shows it to carry very little fibre indeed up to at least one inch or an inch and a half above soil level, so that little fibre would be wasted by close cutting the crop. Flax easily gets tangled and cutting would certainly present difficulty for that reason but this does not appear to be the reason for the statement that flax must not be cut. The explanation seems to centre around the belief that the cut ends of the fibres do not come together kindly when being spun. The main advantage of pulling over cutting seems to lie in getting up the crop more or less free from weeds. Under certain conditions this certainly may be an advantage but seeing that at a later stage, when the seed is separated from the straw, an equally good opportunity is afforded of getting rid of weeds and grading the straw into bundles of uniform length, it seems to be doubtful economy to hand-pull the crop.

Flax is pulled only during dry weather. It is grasped rather low down on the stem in small handfuls and is pulled up with as few weeds as possible, the earth is knocked off from the roots against the puller's boot and, keeping the root ends level, a large handful is accumulated until no more can be held. These large handfuls are laid down on the ground for women to collect together, "even up" and tie into larger bundles or sheaves by twisting a few of the straws round them just below the seed bolls.

The practice in the Russian flax-growing districts is to pull the crop greener than in Holland and it is less carefully handled. In the Baltic provinces, as the bundles are tied up they are collected in a part of the field where a large knife is erected for cutting off the seed bolls and for trimming up the sheaves by slashing them down on to the knife.

In Ireland a somewhat different practice obtains; the pullers themselves lay the uprooted flax neatly across twisted rush bands, until sufficient has been collected to tie up to form a sheaf or as it is called locally, a "beet." As no attempt is made to save the seed, there is no opportunity for "evening up" the sheaves after they are once made up, so it becomes of the greatest

importance to have the flax tied up uniformly in the first instance.

There is much disagreement as to the merits of green-straw retting over dry-straw retting, when regarded simply as a means of preparing the best quality fibre, quite apart from the question of saving seed, because it will be shown subsequently that in either case the seed may be saved.

In Ireland, parts of Russia and certain localities in Belgium green-straw retting is advocated as being the better method, the fibre prepared in this way being, it is said, of superior quality ; on the other hand, the best fibre of all comes from Belgium and is prepared from straw which has been not only dried well but has been kept until the following year before being retted. The character of the growing season, the temperature and nature of the water in which the straw is retted, all play a more prominent part in determining what class of fibre will be obtained eventually, so that it is difficult to ascribe distinctive merit to either method of retting. Judging from information acquired in the different districts, it may be that both methods have some particular advantage ; possibly green-straw retting favours the production of a fibre which is fine and more silky in character and the dry-straw method produces a fibre which is stronger than the other but the evidence in favour of this view is not very conclusive.

To allow the "after-ripening" of the seed to take place the crop is left in the field to dry for a day or two. The Belgian farmer then lays the sheaves uniformly in one direction so as to build up a wall which is propped at frequent intervals to resist wind pressure and roughly thatched with rye straw. By this arrangement, the flax straw is protected from rain and from sun and at the same time the wind has a fair chance of penetrating the wall, so that after some seven or eight days it becomes sufficiently dry for the seed to be removed. The custom in Holland, especially in Groningen and Friesland, is somewhat different from that in Belgium. In the former province, after preliminary drying, the sheaves are built around a roughly constructed wooden tripod, such as is used for drying clover ; they are then left for about a week for the seed to mature and dry. In Friesland the sheaves are made up into small ricks, which are protected at the top by a cloth covering or a light thatch of green rushes.

In some parts of Russia, where the climate is wet, consider-

able difficulty is experienced in drying the crop: rain and inclement weather generally set in before the operation can be accomplished in the ordinary way. To overcome this difficulty, large drying sheds with open sides are erected which are fitted with lattice shelves upon which the flax is laid as soon as it is pulled. Again, in the neighbourhood of Rsheff, after the crop is pulled and has been allowed to dry out of doors as far as the climate allows, it is removed to a drying house, where it is artificially dried in an oven before the seed is taken off.

There are numerous methods of separating the seed from flax straw. Ordinary machine thrashing is strictly avoided, if the straw is to be of much value for subsequent retting, because this method occasions serious damage to the fibre. The method most generally used is that known as "rippling," which is effected by drawing the top part of the straw through a vertically placed iron comb which does not allow the seed capsules to pass between the closely arranged teeth. Men do the actual rippling and women and children bring the sheaves, untie and retie them again. To avoid loss of seed, rippling is carried out over a large cloth spread upon the ground; when the crop is stored until the next year, the rippling is done in the barn in which the straw is housed during the winter. This operation of rippling affords an excellent opportunity of taking out any weeds as well as of grading the straw into bundles of approximately uniform length ready for steeping. Besides being a good practical method of removing the seed, rippling has much in its favour as a means of straightening out the straw and cleaning it from short pieces as well as from weeds. Some go so far as to say that this would be a profitable expenditure even if the value of the seed alone did not completely cover the cost of rippling.

Flax grown in Belgium is sometimes rippled as soon as it is pulled or, after being well dried, the crop is deprived of the seeds it carries by spreading it on an even stone floor and then beating the top ends of the flax with flat wooden mallets. It is quite the practice in West Flanders, especially during the winter months, to effect the removal of the seed by this method. Without having the advantage of straightening out and cleaning the straw, this method of seed separation seems to necessitate the employment of as much labour as does rippling; moreover, it is doubtful whether the seed does not suffer under the treat-



FIG. 1 Harvesting flax Bedfordshire, 1912



FIG. 2 Rippling flax seed Groningen

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ment. The one advantage seems to be that the seed is threshed out and the capsules separated by the same operation.

In localities where flax straw is retted while in the green state, as soon as it is pulled, a practice which obtains in the neighbourhood of Lokern and St. Nicholas in Belgium, the seed capsules are "rippled" off and then spread out on canvas in the sun to dry.

The Russian methods of separating the seed from the straw also vary. In the Baltic provinces and the Government of Pskoff a modified form of "ripple" is employed, in which the teeth are sharp knife blades which cut off seed-pods and the small branches to which they are attached, leaving only the straight stems. Different methods of removing the seed are practised in other parts of Russia; for example, the artificially dried flax straw is taken by the root end in handfuls at a time and just the top ends are passed between the butt ends of the revolving wooden rollers fixed at such a distance apart that the straw is practically untouched and yet close enough together to crush the seed capsules and to free the seed without damaging it.

It has been mentioned already that the general practice in Western Russia is to cut off the top branches and the seed capsules from the flax straw; these are collected together and closely packed on a vertical drying frame erected in the field, where they remain until the seeds within the capsules have become of a uniform brown colour. After drying on these frames out of doors, the seed is removed to a specially constructed drying shed, where it is heated to a fairly high temperature until quite dry: an operation which sometimes lasts during two or three days if the out-of-doors conditions were not favourable to drying.

The seed is then spread rather thickly over a stone floor and threshed, either with a flail, by simple machinery constructed of wood; or a horse is made to drag a grooved wooden roller about the floor. Finally the seed is shifted and screened and then sold to the local buyers, who pass it on with their other purchases to people who properly clean and "grade it for export," whatever that may mean exactly.

In Holland it is customary to separate the seed from the straw by hand labour during the winter months by rippling and sometimes this is done by means of a machine known as a "flax-brake." The seed is very carefully threshed out and

cleaned and prepared for market by the farmer, who relies upon his "Riga-Child" seed making a good price—there being a large demand for this variety of seed by French, Irish and Belgian growers. Most of the French and Belgian seed is sold for oil.

SEPARATION OF THE FIBRE

Before the harvested straw can be of use to the spinner in the customary way, it has to be put through several somewhat complicated processes, including retting, breaking, scutching and heckling. All these operations were carried out formerly by the farmer who grew the straw; but of late the tendency has been for these subsequent operations to get into the hands of people who specialise in one particular phase of fibre preparation.

It is now the more common practice for the farmer to sell his standing crop, the purchaser deciding when to harvest and himself taking off the seed. He then sells the straw to somebody who rets it and then it passes into the hands of others who have specialised in scutching and heckling; finally it is bought by a dealer who sorts and grades his purchases and sells in large quantities to the spinners. This procedure is quite general in those districts where the higher qualities of flax are produced and must be regarded as a consequence of these subsequent processes requiring greater skill in carrying them out than the average farmer is able to command.

The first of these after-processes, namely, retting, involves the partial disintegration of the flax straw and for convenience of reference the structure of a flax straw may be briefly described here. When viewed in transverse section, it may be considered as being composed of two parts or concentric rings: a complex cellular system forming the outer ring and a cell structure of greater simplicity forming the inner ring or woody part of the stem. The valuable part of the straw, namely, the fibre, forms a series of irregular bundles almost on the outside of the stem, their exact position being between two thin parenchymatous layers, one of which is just beneath the epidermis and the bounding cutica, the other being adjacent to the cambium. This briefly describes the formation of the outer layer the complex cellular system of which has to be partly broken down before the bundles of fibre can be obtained in a useful form. The inner part of the stem is made up of a ring of woody material of more or less uniform character and with this

the fibre-winner has little to do. The long fibres composing the "bundles" already mentioned are themselves made up of long chains of shorter fibres which are held together and in position by an inter-cellular gum or resin (pectose).

Successful separation of fibre from flax straw depends upon the isolation of the long fibres without going so far as to weaken the binding between the smaller, individual fibres composing them. Up to the present time, this pectose decomposition has been accomplished best by a natural fermentation process which sets in when the damp straw is allowed to rot: a process which now goes by the name of "retting."

Of the various ways of effecting this decomposition, the simplest is that known as "dew-retting," the straw being spread thinly in regular rows over the ground and alternate dew, sunshine and rain allowed to carry the process forward until the fibre is easily detachable from the wood. The very nature of this process, depending as it does upon favourable weather conditions, frequently gives rise to a product of low value: nevertheless, in some districts, this method is the only one which is possible and enormous quantities of dew-retted flax are prepared annually. One acre of standing flax requires nearly two acres of land over which to spread it and there it remains for two or three weeks. It is then turned over carefully and left for three or four weeks longer, although the time required depends upon prevailing weather conditions. Fibre from dew-retted straw is usually of bad colour although it bleaches well. Sometimes in Belgium, more often in Russia, winter retting is practised, the flax straw remaining out in the field for some months without suffering much harm and the fibre ultimately obtained is of pale colour. In Western Europe only the poorer qualities of straw are dew-retted: crops which are not considered good enough to treat by other and more costly methods.

A method of retting only seen in South Holland and East Flanders is to pack the deseeded undried straw into long, narrow ditches containing some two feet of water and then to cover the whole mass with sufficient mud taken from the pit, so as to completely immerse the straw and prevent it rising above the liquid during retting. Like other fermentation processes, retting proceeds more quickly during warm weather and as this method is carried on immediately after harvesting the crop in July it

only requires from eight to ten days for the straw to be sufficiently decomposed. Experience tells when it should be removed and then the people employed get into the pit and carefully remove the bundles from the mud and water. Needless to say the work is exceedingly unpleasant, more especially because of the powerful stench which arises when the bundles of straw are disturbed. After rinsing in cleaner water, the straw is spread over a stubble field and there it remains for a month or six weeks before it is dried and taken to the barn. The small farmer carries out all these processes himself and although his methods of cleaning the fibre are quite primitive the product he obtains has a good name for softness and pliability. It is dark in colour, inclining to blue—giving the name Blue Flax—but it bleaches easily and is sought after for certain purposes.

Of the retting processes which are still carried out by the farmer, "pond-retting" is the best. This is practised in Ireland, France, Friesland and Russia with considerable success. It involves placing the tied-up bundles of straw in water and allowing them to remain there until properly retted. There are two distinct methods of water-retting—the straw being either floated or submerged: of these the former is the older and at the present day is carried on only in Friesland. The bundles of rippled and dried straw are floated on the surface of a fairly large stretch of still water and every day they are turned over so that the side which was uppermost and out of the water is placed beneath the water next day. This turning is performed by men on the bank, who use a small prong fixed to the end of a light pole.

By far the better method of pond-retting is to submerge the straw completely. Probably there is no place where this is carried out better than in some parts of Ireland and no place where more good flax is sacrificed to this method than in Russia.

For the most part the retting ponds are simple excavations in the ground with a clay bottom, although some few are roughly paved or have boarded sides. It is almost universally agreed that the best method of filling the retting ponds is to arrange the bundles vertically or nearly so, one row deep, with the root ends downwards. When the pond is completely filled, a light covering of straw, tree foliage or other suitable material is generally put over the flax and on the top of that sufficient stones are arranged to submerge the entire mass uniformly. The progress of retting is carefully watched.



FIG. 3 Retting flax Bedfordshire, 1912



FIG. 4 Retting flax Friesland,

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especially towards the end of the operation, when the straw is examined several times each day. The usual time for steeping is from ten to twelve nights and when the adjudged point has been reached the straw is carefully removed from the pond and spread over grass land or opened out and stood upon end to dry.

When larger volumes of water are used or when the water is allowed to flow slowly through the pond, the colour of the resultant fibre is much paler; and when retting is carried out at the shore of a lake or river, the fibre obtained eventually is almost white.

For the production of high-class fibre, the method known as "double retting" stands before all others. It is practised with greatest perfection in Belgium in the neighbourhood of Courtrai, where since the middle of the last century flax has been systematically double-retted in the River Lys. This river is naturally adapted to retting inasmuch as the water is very slow-moving and the river bank slopes gently down to the water-edge. What probably is the cause of such successful retting in this river in particular is the slow movement of the water and the large amount of organic matter which it carries from towns situated some distance above the portion of its course devoted to retting. Bacterial development under these circumstances, aided by the enormous quantity of flax which is annually retted in the river, has resulted in the exceptionally favourable conditions which obtain at the present day.

The Lys retting period lasts from April 15 to October 15 and during that time the river is practically closed to traffic. For some twenty miles on either side of Courtrai a continuous row of retting crates or "ballons" are to be seen packed close together near to each bank of the river and remarkable activity prevails during the whole period. On the river bank the straw is sorted into heaps of approximately equal length of straw and the various heaps are made up into bundles which are packed closely into the "ballons." Sacking is placed along the open front, an ample covering of straw is spread over the top and the "ballon" is then launched into the river and weighted down by large stones so as to submerge the flax straw. During the summer months the temperature of the river water is about 20 to 25° C. and the first retting occupies nearly a week. As fermentation proceeds the "ballon" rises out of the water and therefore requires its weight of stones to be adjusted from time to time.

At the close of the first retting period the "ballons" are hauled up on to the bank, the flax straw is taken to an adjacent field where the bundles are opened and the straw arranged on end in small open sheaves—"steeples"—to dry. After about three days the dried straw is collected together and is generally given a rest-period of about one month before being sorted over again, made up into bundles and retted in the river as in the first instance. The second retting does not take so long as the first retting, although the time necessary depends upon several variable factors such as temperature, quality of original straw, extent of first retting, etc. To determine precisely when retting should be finally arrested requires very considerable knowledge, aided by careful and repeated examinations of the retting straw. When the conditions are satisfactory, the "ballons" are taken from the river and the bundles of straw are removed and dried after the manner already described.

This fermentation process of retting may be accelerated by raising the temperature of the water in which the flax is steeped: a fact which, although known long previously, was first made use of practically by Schenk (1846) who devised a method of retting flax straw in warmed water. Since then many establishments have been organised and worked on this principle in various countries including England and such retting establishments, generally speaking, met with success. The chief drawback to the successful working of many of them seems to have been want of capital. It is of interest to find it recorded that in 1853 as many as twenty such reterries were at work in Ireland alone and that, of the flax factories in England, those which had adopted retting in warmed water at a central depôt were the last to close down. As recently as 1896 there were two such reterries successfully working in Yorkshire.

It will serve no useful purpose to mention here all the various modifications of Schenk's original scheme nor the vicissitudes through which they passed. At the present time there are flax retting depôts at Bruges, Courtrai, Oenkerk and Appingadam where retting in warmed water is successfully practised and the fibre turned out is of good quality.

At the small factory near Courtrai flax straw is retted in cemented tanks; each one being fitted with a false bottom upon which the bundles of straw stand and beneath which steam-pipes are made to warm the water contained in the tank to 27 to 30° C.

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The straw is twice retted and during each operation the water is changed at least twice. At the retting station at Oenkerk in Friesland there are three pairs of retting tanks which are built of stone and lined with wood and these also are fitted with steam-pipes beneath a false bottom. The temperature of the water is maintained at about 30° C. during about three days and nights—until the straw is properly retted—then the water is run off into a field drain and the straw is arranged in “steeples” to dry.

Near Bruges, there is a larger station than at Oenkerk, where an almost identical plan is adopted; the retting being completed in seventy-two hours. Double-retting is practised at Appingadam Central Rettery, where the retting tanks are arranged in series or batterics of four. The tanks are made of concrete and are each provided with an inlet at the bottom for warmed water, overflow pipes and exit pipes and above each battery of tanks there is a reservoir fitted with steam circulator pipes where the required quantity of water is warmed prior to entering the retting tanks.

Early in the nineteenth century retting was studied from the biological side and it was soon established that it was primarily a fermentation process: it was not, however, until much work had been done on this subject that any further definite knowledge was obtained. In 1868 Kolb put forward views regarding the more exact nature of the retting process, namely, that it was a pectin fermentation process whereby the insoluble inter-cellular substance was removed as soluble products of fermentation, thus allowing the fibre to be separated.

This explanation was warmly contested by Tieghem and others who supported the view that the process involved the resolution of the cell structure and the dissolution of the cellular membrane by a specific anaerobic organism. The investigations of Friebes showed that the flax stems themselves carry a definite anaerobic bacterium of somewhat large size which is active towards the intercellular substance but which is quite inactive towards cellulose; this view is held at the present day, although it is sometimes suggested that there are naturally on the flax stems several species of bacteria which are concerned in the retting.

The recent researches of Stormer (1904) and of Hoffmeister (1905) show that the chief retting organism is not difficult

to isolate but, as at present understood, it is doubtful whether the application of pure-culture methods of retting will be financially possible on a technical scale.

Whatever the method of retting may be which necessitates wetting the flax straw, before the fibre can be cleaned the retted straw has to be thoroughly dried. This is effected either by spreading the wet straw on suitable land or by stooking it up on end to dry.

When properly dried the flax straw is gathered together, tied in bundles and, as with all other stages of flax-handling, great attention is given to making up the bundles evenly; all straws should be straight and the ends should present a brush-like appearance. At all stages great importance is attached to the manner in which the flax is put up in bundles, because if not well arranged considerable loss will result when the fibre is cleaned. The dried straw is stored under cover of a barn or under a good thatch until it is convenient to scutch and clean it during the winter months.

This matter of adequately drying steeped flax is a serious one for the management of retting depôts, because, were it not for the difficulty of drying the wet straw during inclement weather, such depôts could continue retting operations throughout the year. As it is, land has to be set apart as drying ground and used only during part of the year. Various attempts have been made to dry the wet straw under cover, in a current of warmed air and in warmed rooms but the amount of moisture which has to be removed is so great that these methods have not proved commercially successful. The wet straws lie in such intimate contact one with another that the occluded water is difficult to remove. If some more open arrangement could be effected the main difficulty of artificial drying would be overcome.

Before the process of cleaning the fibre is attempted, the brittle, central woody part of the dry straw is broken up into small pieces, so that the fibre may receive as little damage as possible when being cleaned: this preliminary process is known as "breaking." The machines used for this purpose were formerly operated by hand and of very simple construction, consisting of grooved wooden levers or single pairs of fluted rollers between which the flax straw was passed and repassed several times. In Russia, Hungary, Silesia and parts of Friesland hand-breakers are still to be seen but it may be

Fig. 6 Belgian search mill
PLATE III

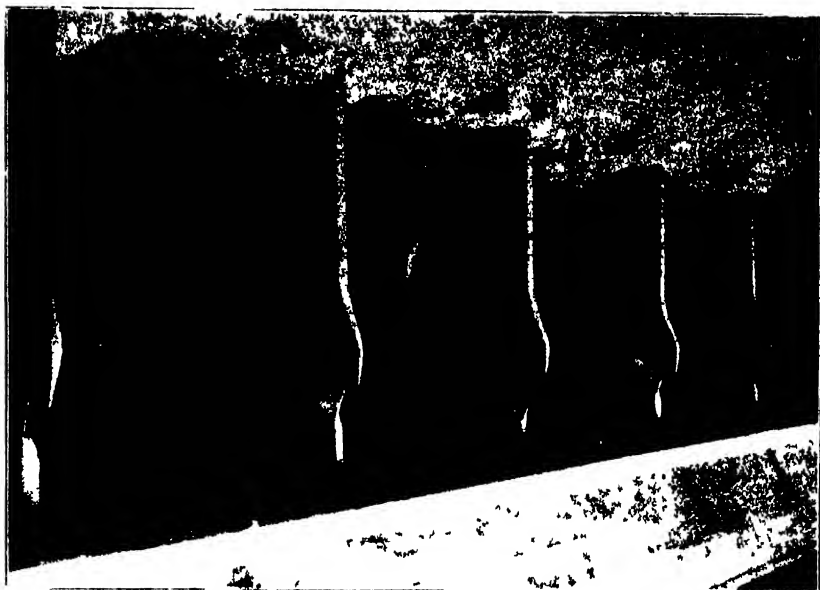


Fig. 5 Iceberg flux in River IJss, near C. where Belgium



said that these appliances have been entirely superseded wherever the flax industry has attained a fairly high level.

Although the principle of the modern machines is much the same as the old-fashioned ones, the "breaker" is now made with many (eight or ten) pairs of metal rollers, some of which are smooth to crush the straw flat, followed by many other pairs of grooved rollers differently fluted; the object being to break up the woody part of the stem and to remove mechanically as much of it as possible at that stage without injuring the fibre. These machines are driven by water, steam or other motive power and ordinarily form part of the equipment of a flax-cleaning mill. The straw is fed into the breaker at one end and received at the other end by lads who handle the material carefully and lay the broken straw in heaps ready for the cleaners.

After coming from the "breaker," the broken-up woody part of the straw—the shove—is separated from the fibre by a mechanical beating operation known as scutching and, save for some details, this is conducted on the principle of submitting handfuls of broken straw to a beating by wooden blades which are either wielded by the hand or are fixed to a rotating wheel.

As a household industry, scutching and cleaning fibre by hand or by hand-driven machinery have quite disappeared except in Russia and some of the more rural parts of Belgium. These simple methods, which admit of varying the treatment at will to suit the particular material dealt with, have much in their favour from the point of view of preparing good fibre: they have, however, been superseded for economic reasons.

The construction of a scutch mill is such that the revolving beaters pass close in front of a rigid upright "stock" over which the flax is firmly held and submitted to rapid beating in a downward direction. The ease with which flax is scutched depends largely upon whether the straw has been well or under-retted: in Belgium, where flax is well retted, the scutching blades are lightly fashioned and the rotating wheel carries more blades than in Ireland, where flax is more often under-retted.

This briefly describes the operation of scutching as carried out almost universally. The methods and appliances are primitive and the treatment accorded the fibre is severe, yet more recent and apparently improved devices for removing the shove have met but slight attention from those engaged in scutching.

in addition to the operations of retting which have been

described already, various other methods of separating the fibre have been advocated from time to time. Although it is not exactly clear why they always fell into disuse, there seems to be good evidence for concluding that it was owing to the dry condition of the fibre obtained, to the removal of the oily and strengthening matters from the fibre which give to it a valuable spinning quality and also to the opposition offered by the manufacturers and the trade generally to a new article.

COST OF PRODUCTION

It is now so long since flax was grown as a field crop in this country that little importance can be attached to the recorded cost of production. Fifteen years ago the estimated cost of this crop in Cambridgeshire, Lincolnshire and Suffolk was said to be about £5 per acre; in Yorkshire a trifle less and in the south of England a trifle more. It is probable that these figures would not represent the cost at the present day owing to the general increase in the cost of production that has taken place during the last decade.

With regard to the preparation of the fibre the same argument applies; moreover, the cost of retting is very variable: frequently in two districts not far removed from one another, the cost of retting in the one may be double that in the other. Scutching is variously estimated to cost about £2 10s. per acre of straw grown but as this depends upon the skill of the scutchers and the extent to which the straw has been retted, the cost of this operation may vary considerably. The most trustworthy information would be obtained from a central rettery where proper records were kept and where the value of the product is recorded. Unfortunately such data are not to be obtained from the few depôts in operation. The only indication of success upon which reliance can be placed is the general appearance of the establishment and the fact that some of them have been in operation for about ten years, during which time modest profits have been made.

It has been mentioned already that during the past year (1912) flax was grown in Bedfordshire as a fibre crop. Certain experiments were made there with a view to getting practical information regarding the successful handling of the crop both in the field and during the after-processes. The field experiments were made to include trials of varieties of seed procured in

Russia and in Holland and the effect of adding muriate of potash at the time of sowing. Different methods of sowing the seed were adopted and trials of different methods of harvesting the crop were made.

Certain points of difference are said to be noticeable when retting is conducted in cement-lined tanks as compared with wood-lined tanks: the nature of the difference in the fibre prepared from undried and from dried straw is not yet understood: likewise the possibility of successfully treating the nauseous tank effluent on a filter bed is unsolved. These and other problems are of considerable importance when the question of centralising retting operations is considered and it was with the object of attempting to elucidate such problems that the experimental tanks referred to were constructed.

So as to avoid having to attribute any success obtained with the crop to exceptionally favourable soil, when selecting the land care was observed not to choose that which was eminently suitable to the flax crop but rather a soil which, if anything, was adverse to its growth. An able farmer of good standing, who farms gault land near to the chalk, was supplied with the different varieties of seed and asked to do his best with the crop, one of the reasons for making the trials being to ascertain what difficulties would be encountered when employing labour which was unfamiliar with the work.

The unusually dry weather during April seriously delayed the sowing of the seed, in fact some of the plots were not sown until well in May. Afterwards, the season became exceptionally wet; rain fell so frequently during August and September that harvesting operations were interrupted and were often completed with difficulty, as was also the drying of the retted straw.

Some difficulty was contemplated in getting the crop weeded and pulled and in this there was no disappointment, although the villagers displayed some anxiety to do their best and their services became more useful as they became more familiar with the work. No difficulty was experienced in getting female labour in the fields, indeed, some women were glad to walk nearly three miles to the work.

At no stage of the growth of the flax nor yet at the time of harvest could any difference be observed between the part of the plots which had received a dressing of muriate of potash

and that which had not. Generally speaking the crops were distinctly good, although in some places the consequence of irregular germination was markedly shown.

With such frequent showers of rain falling, it was found impossible to dry the crop when tied up into sheaves but this was successfully accomplished by stretching a number of wires the entire length of the field against which the flax was lodged as soon as it was pulled. When sufficiently dry the flax was then removed to the shelter of a large rick cloth where women were engaged in rippling off the seed after the manner adopted in Holland.

The deseeded straw was sorted over and tied up into bundles and these were packed vertically in the retting tanks and over them some hurdles were placed upon which rested a heavy piece of timber to keep the bundles in position. Water was allowed to enter the tanks from a neighbouring stream and then a sufficient weight of large stones was distributed over the hurdles to keep the entire mass uniformly submerged.

After about a week had elapsed the water in the tanks was run out into a settling reservoir, fresh water was admitted from the stream and the retting allowed to proceed. Although in the first instance retting in the cement-lined tank commenced later and proceeded at a slower rate than in the wood-lined tank, after the first batch had been retted no such difference was apparent. When the straw was sufficiently retted the tanks were again emptied and the straw was removed to an adjacent field where experiments on drying were made on the lines of those practised in other countries.

The attempts made to construct a filter bed to purify the tank effluent were not altogether satisfactory, although the analyses of the liquor made before and after filtration indicated the possibility of success attending further experiments.

The work done last year took more the form of a preliminary trial of the more difficult operations of flax growing and fibre separation, namely harvesting and retting; experience was also gained in carrying them out under very adverse circumstances. It is anticipated that during the present year it will be possible to make arrangements to study further the problem of purifying the effluent and also to conduct more systematic experiments with a view to ascertaining more exactly what would be the best provision to make for establishing a small retting station.

THE STATE PROTECTION OF WILD PLANTS

By A. R. HORWOOD

Leicester Museum ; Recorder, Plant Protection Section, Selborne Society

IF there be one direction in which the British Isles is particularly behindhand, it is in the matter of preserving and protecting the native flora. This is the more apparent when it is observed that Germany or rather, it should be said, Prussia, has a well-organised State Department for this purpose, whilst we in England have neglected to take any such precaution.

Nor is Prussia the only country that has realised the necessity of giving State protection to wild plants, many other continental nations having adopted this measure and America has also realised its importance. As if to emphasise the need at home, many of our own Colonies have already adopted temporary or partial means of preservation or protection in special cases, by establishing reservations and by other methods.

It is proposed to examine the peculiar circumstances which make State protection necessary in this country and to describe the temporary expedients resorted to already to prevent the extermination of plants.

The principal causes at work contributing to the complete or local extermination of wild plants are :

Smoke ; atmospheric abnormalities ; drainage ; cutting down of woods ; desiccation ; drought ; cultivation ; building operations ; sport ; hawking and collecting ; professional collecting ; nature-study operations.

Dealing seriatim with each of these major factors, the first, smoke, is undoubtedly more potent than most of the others. Industrial activities are continually enlarging the area of operations in which the consumption of fuel is a necessary factor, the effect being to transform completely the character of the open country to the north-east of large towns and coalfields, in fact wherever centres of industry have been established. Cryptogams more especially, as I have shown elsewhere, have exhibited a marked decrease in number and character all over the country.

The effect of fog in London was described by Prof. F. W. Oliver more than twenty years ago and G. Bailey has proved that the same effect can be demonstrated as arising from the aerial conditions in the Manchester district. Glasgow, Birmingham and Liverpool are other cities that are similarly affected by the smoke evil.

Nor do these statements rest alone upon the authority of those whom I have mentioned. Cryptogamists in all parts of the British Isles bear testimony from their own observations to the deleterious effect of smoke. A notable instance is the Black Country, which is almost entirely denuded of cryptogams. The smoke-clouds of Yorkshire can be seen at a distance of thirty miles away and their effect is well known.

The atmosphere itself, apart from its accompanying impurities, has undergone a change which has become particularly marked during the last twenty years, these islands being much drier than formerly.

One of the causes of the incidence of a drier era is undoubtedly drainage. We have only to mention the Fens as an illustration of this process being carried out on a large scale to demonstrate the extent to which a limited area in this country has been drained of its inherent moisture but though less obvious elsewhere, drainage has produced a similar effect in all areas brought under the conditions necessitated by modern methods of cultivation.

The decrease of moisture, which is especially deleterious to hygrophiles adapted to grow only under moist conditions, is indirectly brought about also by the cutting down of trees or woods. Thousands of acres of wood in Scotland, once used as deer forests, have been cut down. In historic times, both England and Ireland were extensively covered by tracts of forest; remnants of these are to be seen to-day in spots where ancient oaks still linger and are pointed to as the trees under which perhaps Druids once worshipped. Cæsar's account of Britain shows that Central England was once a wide region of primæval forest. To-day, with the exception of isolated forests—Sherwood, Arden, Charnwood¹—it is given up to a commonplace mesophytic vegetation and consists largely of pasture or meadow-land.

Intimately allied to the last factor is the cultivation of land.

¹ Prof. H. Conwentz thought that not a remnant of indigenous woodland could be found in this country.

The ridge and furrow of the midlands testify to the former extent of cornlands and illustrate the purely local character of a method of drainage which caused little more than local disturbance of conditions without removing them. They allowed for an alternation of xerophilous and hygrophilous plants without driving out either class.

Where this primitive type of drainage alone persists, what I have ventured to call "vestigial floras" or remnants or indications of the real natural plant-formations will be found surrounded by a modern mesophytic type of vegetation. The insignificance of the vestigial floras affords, in the field, an optical demonstration of the immensity of the changes wrought by this one factor alone, the removal of water by drainage. Where, moreover, land is drained by modern processes, by carrying the water by drains to ditches, thence to streams, lastly to rivers and the sea or lakes, the change is complete. There are not even traces of a vestigial flora—there is in fact no aboriginal flora. Its place has been taken by another type of flora.

If the grass-pastures alluded to are converted into cornfields, there will be fresh changes. And a fresh race of alien plants will impress itself upon the remnants of mesophytic vegetation. This like the preceding phase will be artificial and from the point of view of the continuance of natural plant-formations is an instance of wholesale extermination on a very large scale. And from the scientific point of view, extermination must be examined in the light of the original not the derived or secondary plant-formations.

Another important cause of disturbance and extinction is the extension of building operations. The later extensions and modifications of the City of London have brought about extraordinary changes, as may be proved by comparing Curtis's *Flora Londinensis* with the present flora. The increased attention given to sanitary conditions leads to the alteration or pulling down of old dwellings in old towns; in this connexion their very antiquity is the point of importance. Cryptogams, particularly Lichens and Mosses, are especially addicted to such habitats and are destroyed by the pulling down of old buildings, whilst the erection of new buildings on fresh ground involves the destruction of other habitats, since the sites chosen are invariably the areas occupied by plants not found elsewhere. This is especially the case where towns, as is often

the case, are built on natural beauty-spots or on particularly salubrious sites.

It is perhaps un-British to condemn anything which encourages the love of *sport* but nowadays vast areas are given up to recreation, whereby wild plants on the outskirts of towns are exposed. This applies especially to golf. A certain type of ground, suitable for golf-links, by an irony of circumstances is very favourable to the growth of a class of rare or local plants. And links are artificially treated, so that the natural turf becomes altered in the process and all but the soft grass tends to disappear. The proximity of golf-links to a large city at once effaces the extensive flora that tracts suited for links afford; as an example, we may mention Barnes Common, once noted for many uncommon wild-flowers. Racecourses again are examples of the same correspondence between rare plant habitats and natural features suited to sport. The old racecourse at Leicester afforded before its conversion into a sporting centre a station for the Mouse-tail, a particularly rare plant in this county.

One of the most important factors of plant extermination, because selective, is the practice of commercial hawking and collecting. It is enough to offer, as an example of this class of vandalism, the case of the Killarney Fern, which was sold in Killarney as long ago as 1850 for five shillings a single root. This and other cases of the kind in Ireland I have already described elsewhere. And what applies to Ireland applies with greater force, in regard to the extent of such ravages, in England, Scotland and Wales. Moreover, ferns are not the only commodity in request but many other wild plants, especially the beautiful ones, such as anemones, primroses, bluebells and orchids come within the purview of the hawking fraternity.

To some extent the modern practice of taking holiday excursions has been the cause, in the neighbourhood of holiday-resorts, of the disappearance of the wild-flowers that used to adorn such beauty-spots at the commencement of the holiday-making era. This cause may appear unimportant to the uninitiated but statistics show otherwise.

The districts around towns are not the only source of plunder for this class of depredator, for hawkers and tourists alike invade the more secluded spots where vegetation is luxuriant and take toll of the rarities to be found in such haunts.

These people are not experienced in distinguishing between allied species, nor do they know the habitats (it is to be hoped) of the rarest plants, which is some satisfaction to the person interested in the welfare of our native flora.

Perhaps the scientific collector is the person who does the greatest harm. He possesses the intimate and expert knowledge which enables him to go to the exact spot where rarities grow and to discriminate between closely connected species, a difficult task at best. Whilst the hawker causes wholesale extermination of common plants usually the most beautiful, causing local extinction, the scientific collector collects the rarities in the few spots in which they grow and can ultimately bring about their universal extinction.

The very general attention given at the present time in the elementary schools to nature-study is another likely means by which wild-flowers may be diminished in number. Having regard to the normal desire of the teacher to inculcate a love of nature and at the same time to impress upon his pupils the necessity of regarding the beauties of the countryside as a treasure not to be misused, it may be hoped that there is not any need to fear widespread difficulties from this cause; but the possibility exists and must be guarded against, as the young mind has no idea of taking thought for the future.

There are a considerable number of minor causes at work contributing to bring about the diminution or extinction of species, locally or universally, in the British Isles but it is not our present purpose to consider these, as they have been dealt with elsewhere. The consideration of the main causes enumerated is assuredly enough to make it necessary to discuss the possible remedies that at present lie to hand.

The general character of many of the factors which lead to the extinction of plants requires that any remedies that may be introduced should be comprehensive, wholesale, effective and permanent. Moreover no remedied measures will have any of these qualities unless they also carry authority.

It is needless to suggest that the most effective means will be the establishment of State protection.

It should be some incentive to us in this country to work towards this ideal, that, as mentioned already, the Prussian Government has a well-organised department of the State

charged with the preservation and protection of natural monuments. And we would ask, if this be possible in Prussia, can it not also be made an accomplished fact in England? The more or less general adoption of some means of preservation by other European and foreign nations, as well as by our own Colonies, should be reason for action on our part.

The present efforts to foster a movement towards the State protection of plants have been primarily guided by the importance of educating the public as to its need.

Towards the close of 1910 an arrangement was made whereby the campaign which I had hitherto carried on personally was made the special objective of a section of the Selborne Society. The Society has always regarded the welfare of plant and animal life as part of its programme from the commencement of its career; but hitherto its activities had found an outlet in other channels.

At the suggestion of my friend and former tutor Prof. G. S. Boulger, therefore, a section was initiated, called the Plant Protection Section, with Dr. A. B. Rendle as Chairman, myself as Recorder.

It is proposed to give a summary of the work and aims of the section and at the same time to consider remedies that may sooner or later be adopted for the factors of extinction discussed in the previous section, taking them as before one by one.

With regard to the influence of smoke, it should be remembered that there is a Smoke Abatement Society at work in a great number of our industrial centres. It is not, however, universal and has not yet acquired a national character. The Black Country and the coalfields are exempt from the control of any smoke regulations.

But in so far as private consumption of coal is concerned, the tendency is rather towards economy and the adoption of smokeless fuel. The construction of smokeless grates is receiving increased attention and herein lies some hope of the arrest of the smoke evil.

The dryness of the atmosphere can be remedied in at least one direction which will be productive of good in more than one way. The cutting down of trees may be counteracted by reafforestation—a practice on the increase.

It is a promising feature to note that the Woods and Forests Department has at last recognised the necessity of training

foresters with a view to the proper care of our national forests. This must have a beneficial effect upon the forests and woodlands in private hands by encouraging a wise and skilful supervision of those sources of fuel and moisture also. It is the retention of the latter that we specially advocate here but as it is intimately wrapped up with the preservation and establishment of permanent woodlands the encouragement of the latter aspect is the one to emphasise, because one of more direct economic importance. The encouragement of the keeping of Arbor Day has always been advocated by the Selborne Society and it is now receiving wider recognition, so that children may be impressed with a desire to be provident in this matter.

The desiccation which is due to drainage is a question which is best dealt with by the advocacy of a general system of irrigation. There can be no two opinions as to the value of this practice and of the necessity of adopting it in this country, especially since the recurrence of droughts periodically has become an established fact. The necessary adaptation of moisture-loving plants to xerophilous conditions can only be controlled, to the advantage of the hygrophiles, which with difficulty survive this artificial struggle for existence, by the reservation of typical areas required by such hygrophilous species; and reservation is again a matter for State organisation.

Coming next to the increasing demolition of buildings, especially ancient ones, it is a matter for satisfaction that in the National Trust for the Preservation of Ancient Monuments and places of natural beauty we have a body actively engaged in the acquirement and preservation of such sites. Moreover, the recent recognition of the value of the work done by the National Trust by the State in the proffer of advice in such matters by the Office of Works is a good augury for the future not only of this phase of preservation of monuments but also for the existence of a department for the protection and preservation of all natural monuments, as in Prussia. That other bodies, such as the Kyrle Society and Commons Preservation Society, as well as the Footpath Associations, are receiving public support on a wide scale shows that there is ample scope for optimism in this direction.

Moreover the care of the highways is another matter requiring urgent attention. Hedges and ditches of roadsides and paths are being periodically despoiled of their beauty by the

operations of the roadscraper, hedgecutter, macadamiser and others. The influence of motor-cars which bespatter the highways with dust and oil is another disquieting feature. In this case the Plant Protection Section is endeavouring to elicit the sympathy and support of the rural and urban district councils to abolish the formal treatment of roads and to regulate the motor traffic.

As to sport, it is necessary to arouse the interest of the great landowners in the value of plant-life so that they may be led to favour the subordination of golf-links made upon their property to the natural features of the district and the preservation of wild species of plants. The Selborne Society here again aims at influencing both landowner and sportsman. In the case of racecourses near towns, it is necessary to approach town councils as to any encroachment of these upon natural features. In the case of public parks, the parks committees need advice as to the conversion of natural features into artificial recreation grounds. In this, as in other matters, the active support of the public is required.

To put a stop to the practice of hawking wild plants is a work that can only be accomplished by the aid of the county councils. Some of these, as in Essex, Devon, Surrey, have already framed byelaws for the prevention of hawking on the highways and property over which they have control. The Selborne Society aims at obtaining the promise of every county council to follow suit. Having accomplished this, it will be an easy step to legislate, the next stage towards State protection. By this means private property not under the jurisdiction of the county councils would be safeguarded in the same manner as the highways.

Already a Bill has been drawn up by Prof. Boulger, which has the approval of Lord Avebury; one of the next steps is to introduce it into Parliament, on the first favourable occasion.

The vandalism of the hawker, of which more is heard than of the other equally deleterious factors of extinction, can be considerably prevented or controlled by the aid of the scientific societies in the country. It is proposed to ask each of these bodies to appoint one of their members to act as a corresponding secretary and local representative, keeping the Section in touch with local requirements and possibilities of support.

One of the methods of opening the eyes of the public to the

gravity of the situation is to publish leaflets setting forth concisely the losses in prospect and appealing to their common sense to prevent the vandalism which goes on. Through county councils and others fifty thousand such leaflets have been distributed appealing "to the public" and "to teachers of nature-study." Cards to be hung up in public places have also been distributed. The assistance of the clergy and medical profession is to be enlisted in this work. The influence of the Press in drawing public attention to the matter is also to be sought.

Another important means of strengthening the evidence for the adoption of State protection in this country and of promoting its realisation will be to secure the co-operation of affiliated bodies, such as the British Association, Yorkshire Naturalists' Union, South-Eastern Naturalists' Union and others. The attachment of the Woods and Forests Department, the Board of Agriculture and Board of Works to the cause will further strengthen the hands of those who wish to promote State protection.

The danger that may result from the pursuit of nature-study is only to be counteracted by the co-operation of teachers and the issue of leaflets discouraging excessive collecting. This has already been done and we believe with beneficial effect. The readiness with which the county councils undertook the work of distribution promises well for the proposed appeal to them to frame byelaws against hawking and in other ways help on the cause.

Over-collection in the schools may be guarded against by the establishment of school-gardens, a step which in itself will definitely encourage the study of botany.

Moreover museums are rapidly beginning to lay themselves out to provide wildflower tables for the public by the aid of which botanical study is given a direct stimulus and a certain economy of material is secured, whilst at the same time quite as much information is conveyed as when several separate collections are made in different schools.

Akin to this method is the formation of a wild garden in the proximity of the school itself, the seeds sown being collected in the district during the autumn of the previous year.

It is the opinion of the Plant Protection Section that, if these and other methods are adopted, some, if not a great, measure of success will follow the efforts to preserve the native flora of the British Isles by the creation of a department of the State to carry out proposals such as are made in this article.

FURTHER SPECULATIONS UPON THE ORIGIN OF LIFE

By CHARLES WALKER, D.Sc.

THE specialisation which has been the inevitable result of the enormous increase in the general fund of knowledge during the past sixty or seventy years is rendered very evident in the recent discussion on the origin of living matter ; it appears to be impossible, at the present time, for a man to possess more than a superficial acquaintance with any branch of science excepting that to which he has devoted himself particularly. So great is the accumulation of recorded observations that, as a rule, it is possible to keep up to date only in one section of one of the great branches of scientific knowledge ; yet to consider this problem properly it is necessary to call in the help of biology and chemistry in some of their latest stages and probably also physics.

It appears to me that in this discussion each biologist has placed the solution of the problem where he sees the fewest difficulties are to be faced ; this moreover has not been where his knowledge has been most detailed and intimate. Such a course is a very natural one to adopt and I shall be obliged to follow to some extent the example of better men and do the same thing myself.

Upon one point biologists seem to be more or less agreed—that the problem is fundamentally one for the chemists. Chemists, however, are not unanimous, I notice, that the biologists have done enough of their share of the work to place them in a position to state the problem in such a manner that it can be handled by the chemists.

As has always been the case in such discussions, metaphysical conceptions have been offered as explanations by several biologists. Our knowledge of the properties of living matter and of the possible conditions under which it may have originated has always been hindered, never helped, by metaphysical conceptions, from those propounded by the author of

the Book of Genesis down to those advanced by Driesch, Bergson and others. I therefore propose to leave vitalistic ideas alone and to begin by glancing at certain pertinent points relating to some properties of living matter which are common to the overwhelming majority of organisms belonging to both the animal and vegetable kingdoms.

The unit of living matter, as far as we know, is the cell. I will not at present try to give a comprehensive definition of a cell but will deal, for the moment, only with that form in which it is found in multicellular and the majority of unicellular organisms both animal and vegetable.

The cell, in this sense, is a mass of protoplasm generally so small as to be invisible to the naked eye. In some cases it is surrounded by a covering, which apparently may be formed from a secretion or excretion and have ceased to be a living part of the cell; or it may be a membrane formed from the protoplasm which continues to live. In other cases, the cell is said to possess no covering but there are many observed facts which make this assumption unacceptable. Whether there is or is not always either a membranous covering or a layer of differentiated protoplasm which acts as such is not material to the point of view from which I am dealing with the subject under discussion.

Within the mass of protoplasm—the cell—is an area surrounded by a membrane which differs in several ways from the rest of the cell. This is the nucleus. The rest of the cell is known as the cytoplasm. When cells are fixed and stained, it is found that within the nucleus are collections of a substance which has a great affinity for basic stains. On account of its taking up stain very readily, this substance has been called chromatin. Chromatin generally appears as minute granules, sometimes collected together in masses of varying size, sometimes arranged in strings. The most usual form is a combination of masses connected by a meshwork of strings. The chromatin appears to be always enclosed in an envelope of an apparently homogeneous and not readily stainable substance known as linin.

Within the nucleus are usually found either a single or several more or less rounded bodies, the nucleoli. These generally differ to some extent from the chromatin in their behaviour towards stains.

In the cytoplasm of cells, excepting those of the higher plants, a pair of minute bodies known as centrosomes is found. These bodies play an important part of which I shall have to speak presently in cell division. They also appear frequently to be connected with the motile appendages of cells. Besides these there is a group of structures in the cytoplasm known as chondriosomes, which are further subdivided according to their structure and appearance.¹ At present, however, we need only consider them as a single group. Apart from certain phenomena connected with chondriosomes which I shall deal with later, it is only necessary to say that they give rise to the fundamental material from which are formed the specific cytoplasmic substances found in the cells of various tissues, such as certain parts of the striped muscle fibres and "prickles" in the cells of the skin.²

Cells, as far as we know, have but one mode of origin and that is from preexisting cells. The way in which cells divide is by no means simple. The chromatin in the nucleus becomes collected into a number of well-defined bodies, generally in the form of U's and V's, which are known as chromosomes; these bodies divide individually, splitting lengthwise, thus ensuring a division which is both quantitative and qualitative. While this is happening, radiations appear in the cytoplasm around the centrosomes, some of the radiations running between the two. The centrosomes separate further and further apart, until they are found at opposite poles of the cell with a spindle of radiations extending between them. The nuclear membrane breaks up and disappears, each chromosome becoming attached to one of the spindle fibres. At the same time the cytoplasm takes an hour-glass shape and the half of each chromosome travels towards the opposite poles of the cell, so that when the constriction in the middle of the hour-glass terminates in the separation of the cell into two daughter cells, an exact representative half of every original chromosome is present in each.

¹ Meves, F., "Die Chondriosomen als Träger erblicher Anlagen. Cytologische Studien am Hühnerembryo," *Arch. f. Mikro. Anat.*, Bd. 72, 1908.

² Duesberg, J., "Les Chondriosomes des cellules embryonnaires du poulet, et leur rôle dans la genèse des myofibrilles," *Arch. f. Zellforschung.*, Bd. iv. 1910; Arnold, G., "On the Condition of Epidermal Fibrils in Epithelioma," *Quart. Journ. Micro. Science.*, vol. 57, part 3, Feb. 1912; Firket, J., "Recherches sur la genèse des fibrilles épidermiques chez le poulet," *Anat. Anz.*, Bd. xxxviii. 1911.

Every multicellular organism begins its existence as a single cell, which in most cases is formed by the fusion of two cells, one derived from each parent. This cell divides into two; each of these divides in turn and so on, until the whole body of the organism is built up. Remembering what happens in cell division, it is clear that every cell in the body, including those that are to be cast off eventually to fuse with other cells and form new individuals, must contain exact representatives of the chromosomes contributed by the parents. This has led to the very general assumption that the chromatin is the determinant of the hereditary characters, the actual substance by which these are conveyed.¹ The sexual act is held to consist essentially in the union of chromatin from two distinct organisms.²

With these views I disagree most emphatically. To begin with, it seems quite possible that the chromatin is merely a secretion of the linin. It waxes and wanes at different times in the same cell, particularly during certain periods preceding division. If, however, it be true that chromatin is only a secretion of the linin, the same claims would doubtless be made for the latter substance. Unfortunately we know but comparatively little concerning it, beyond the fact that it forms an envelope around the chromosomes, around the masses of chromatin in the nucleus when in the vegetative state, a meshwork between these masses; also that it probably gives rise to the nuclear membrane, in some cases at any rate. However, in view of recent observations and experiments, neither linin nor chromatin can be claimed as the sole or even chief means by which hereditary characters are transmitted; nor can the importance of the fusion of two cells which constitutes sexual reproduction lie solely in union of the chromatin from two distinct organisms.

It has been shown that the chondriosomes divide individually just as do the chromosomes. This has been traced from the first segmentation of the ovum up to a late stage. They are carried in the cytoplasm of the sperm and fuse with those in the ovum, eventually forming the specialised cytoplasmic structures

¹ Strasburger, Hertwig, Kolliker, Weismann and others at different times have advocated this view. See *The Cell*, Wilson, E. B., 1904; *Heredity*, Thomson, J. A., 1908; Minchin, E. A., *SCIENCE PROGRESS*, Oct. 1912.

² Weismann's theory of Amphimixis. Minchin, E. A., *SCIENCE PROGRESS*, Oct. 1912.

found in the various kinds of somatic cells.¹ Chondriosomes have been demonstrated in every class of cell in which they have been sought.² Though often difficult to demonstrate, owing to their not being easily stainable by the methods generally used, I have been able to find them in every kind of cell, animal or vegetable, in which I have looked for them. Here then are cytoplasmic structures which are handed on from cell generation to cell generation, for which claims as the transmitters of some of the hereditary characters may be made as logical as are those made for the chromosomes.

Enucleated eggs of one kind of animal have been fertilised with the sperms of another kind and in spite of a total absence of maternal chromatin and linin, the resulting embryos have shown purely maternal characters.³ When certain parts of the cytoplasm of the ovum are removed before segmentation, it has been shown that in the resulting larva certain parts of the body are absent.⁴ This is most significant, for it must be realised that as all the cells constituting the fully developed organism arise from the single cell—the ovum—if the destruction of a certain portion of the cytoplasm of this cell result in the non-appearance of a certain group of cells in the developed organism, the power of producing the particular differentiation found in the group of cells involved must have been latent in the cytoplasm and not in the nucleus.

¹ Meves, F., 1908, *op. cit.* "Über die Beteiligung der Plasochondrien (Chondriosomes), an der Befruchtung des Eies von *Ascaris megalocephala*," *Archiv für Mikro. Anat.* Bd. 76, 1911; "Meves and Duesberg, Die Spermatozytenteilungen bei der Hornine," *Arch. f. Mikro. Anat.*, Bd. 71, 1908; Duesberg, *op. cit.*, 1910, "Sur la continuité des éléments mito chondriaux des cellules sexuelles et des chondriosomes des cellules embryonnaires," *Anat. Ans.* Bd. 35, 1910; Arnold, *op. cit.*, 1912; "The rôle of the Chondriosomes in the cells of the guinea pig's pancreas," *Archiv. für Zellforsch.*, 8 Band 2 Heft, 1912; Firket, *op. cit.*, 1911.

² St. George, V. la Valette, "Spermatologische Beiträge," *Arch. f. Mikro. Anat.* Bd. iii. 1886; Benda, C., *Verh. d. phys. Ges. zu Berlin*, 1896-7, 1898-9; *Ver. d. anat. Ges. Kiel*, 1898; Hoven, H., *Arch. de Biol.*, vol. xxv. 1910; Faure-Frémiet, *C. R. Soc. de Biol.*, 1909; Prenant, A., *Journ. de l'Anat. et de la Phys.*, vol. xlv., 1910, and many others.

³ Godlewski, E., "Untersuchungen über die Bastardierung der Echniden und Crinoidenfamilie," *Archiv für Entwicklungsmechanik*, Bd. 20, 1906.

⁴ Fischer, A., "Entwicklung und Organdifferenzierung," *Archiv für Entwicklungsmechanik*, Bd. 15, 1903; Wilson, E. B., "Experimental Studies on Germinal Localisation," *Journal of Experimental Zoology*, vol. i. 1904; and many others.

It is therefore probable, if not certain, that chondriosomes and perhaps other constituents of the cytoplasm play an important part in the transmission of hereditary characters. I do not for a moment mean to imply that the part played by the chromosomes is not an important one. The point I wish to make is that, at the present time, in view of recent work, no one has any right to make the exclusive claims for them that were considered justifiable in the past and which are still considered valid by perhaps the majority of biologists. I have elsewhere given the details of a possible interpretation of the relative parts played by the chromosomes and other portions of the cell with regard to the transmission of hereditary characters; or, to speak more correctly, of the potentiality for developing these characters.¹ Here I only wish to show that this function cannot possibly be confined to the chromatin.

The next point is the relative importance of nucleus and cytoplasm. Here the very general opinion of biologists is that the nucleus is of supreme importance, the cytoplasm playing but a subsidiary part. With this opinion I am again at variance. The nucleus *qua* nucleus is of no more importance than the cytoplasm. Prof. Minchin says that a portion of cytoplasm without nucleus cannot survive. Quite so, but it also seems that neither can a portion of nucleus survive without cytoplasm. Verworn, whose experiments upon the protozoa with regard to this point are among the most important,² came to the conclusion that the one was as important as the other; neither could survive alone, whilst from a small piece of nucleus together with a small piece of cytoplasm a whole organism might be formed.

Much stress is laid upon the fact that in some cells, notably sperms, the nucleus and its contained chromatin form so large a part of the whole; it is therefore concluded that the cytoplasm is of little or no importance. In view of the facts I have already adduced, I feel that the actual relative volumes of nucleus and cytoplasm are not of fundamental importance with regard to the subject under discussion. The nuclei of ova are as small relatively to the whole cell as those of sperms are large. The cytoplasm of the ovum has, of course, to provide nourishment during a greater or less period of time after fertilisation and

¹ Walker, C. E., "Hereditary Characters" (Arnold, London, 1910).

² Verworn, M., "Die physiologische Bedeutung des Zellkerns," *Archiv für die gesammte Physiologie*, ii, 1891.

his accounts for some of the difference. But still there is a certain proportion of cytoplasm in the sperm and that has nothing to do except combine with the cytoplasm of the ovum. With this cytoplasm go chondriosomes which fuse with the chondriosomes of the ovum.¹

Again, in many cells, the nucleus is so small in comparison with the cytoplasm that it long escaped the notice of the microscopist. So much so that we still read in histological descriptions of a structure being "cellular" in contrast with adjacent living structures and this in spite of the fact that all the structures described are composed of cells and nothing but cells, though the nuclei may be so small as to escape any but the most careful examination. The relative bulk of nucleus to cytoplasm would appear to be determined in each case by adaptation to the immediate environment of the cell.

I am quite unable to accept the idea that the nucleus alone produces enzymes. Digestion of particles that have been engulfed always takes place in the cytoplasm and as I shall describe shortly, there seems to be at times a special provision against the cytoplasm having a chance of acting directly upon the nucleus. All the phenomena that have been described as taking place within the cell as connected with the production of enzymes occur in the cytoplasm and the granules which are connected with these secretions are stated to be derived from the chondriosomes, which in turn have been derived from the chondriosomes of the gametes.

It is claimed that some organisms, particularly certain bacteria, consist of nucleus only without cytoplasm. This I feel is a somewhat dangerous claim. Cytoplasm has been demonstrated in many bacteria and when the methods of preserving and staining bacteria become more refined, it seems eminently probable that a thin layer, at least, will be found in all. It is, after all, only a few years ago that parasitic protozoa were always fixed by drying them upon a glass slide, generally by means of violent heat. When the extraordinarily delicate structure of these organisms is considered, it seems wonderful that so much was discovered in spite of this barbarous method. Bacteria are more resistant to rough treatment than are protozoa but still it is too soon to make such a definite statement as that some of them have no cytoplasm. Besides this,

¹ Meves, F., 1911, *op. cit.*

it is quite reasonable to regard bacteria and other unicellular forms such as spirochætes, in which the chromatin appears to be excessive in proportion to the whole body, as organisms specially modified from ancestors that were more primitive for particular conditions of life. Moreover, some cytoplasmic structures take basic stains in a manner very similar or identical to the chromatin.

Perhaps the most important point of all regarding the relations between nucleus and cytoplasm is the fact that we have conditions in which there is no definite nucleus. At certain stages in the life cycles of some organisms the chromatin is distributed throughout the cell. It is probable, personally I feel certain, that these small masses of chromatin are always surrounded by linin but this does not affect my argument. Also, when any cell divides in the manner described above, it is obvious that the ground-substances of the nucleus and of the cytoplasm are inextricably mixed together. Round the chromosomes there is always an envelope of linin and the same or something of a similar nature may possibly exist in the case of the chondriosomes. It is evident that the chromosomes, the chondriosomes and the nucleus must be surrounded by something which is impervious to those substances present in the cell which cause the disintegration of organic matter. It may well be that this is a property of linin.

It therefore seems to me clear that a differentiation into nucleus and cytoplasm is probably not indispensable for the life of all cells. It would appear rather to be a differentiation which has been brought about by natural selection from a more primitive condition. This differentiation disappears temporarily during division in all cells. Except during division, an exchange of substance takes place between nucleus and cytoplasm through the extrusion of the nucleoli. When the cell is in a vegetative condition the nucleoli multiply continually and are extruded from the nucleus. The nuclear membrane is pushed out in front of the migrating nucleolus and closes up behind it as it passes. The staining reaction of the nucleolus changes directly it reaches the cytoplasm.¹ All processes of digestion, absorption and of specific secretion,

¹ Walker, C. E., and Francis M. Tozer, "Observations on the History and possible Function of the Nucleoli in the Vegetative Cells of various Animals and Plants," *Quart. Journ. of Exper. Physiology*, vol. ii. No. 2, March 1909.

take place, as far as we can see under the microscope, in the cytoplasm. The process by which the nucleoli are extruded is such that the cytoplasmic substance which is capable of disintegrating organic matter does not get access to the nuclear substance. It appears not improbable that the differentiation into nucleus and cytoplasm is a definite separation of different functions of the cell into different areas, just as the functions of the liver and kidneys are a localisation of certain functions in different areas of the body and different groups of cells. Both are the outcome of more primitive conditions.

If these arguments are valid, then a differentiation into nucleus and cytoplasm is not essential to life. All that appears to be necessary is certain centres of activity existing in what is apparently the only suitable medium—protoplasm.

This very vague statement takes us no further without some enlargement. We have good evidence that there may be in cells actual centres of activity of the most fundamental importance which have no apparent morphological structure and can, in fact, only be demonstrated indirectly. We saw, when considering the phenomena of cell division, that the centrosomes formed the centres of two sets of radiations, the which radiations formed the spindle fibres to which the chromosomes became attached, the centrosomes forming the poles of the division figure. No one who has studied cell division at all adequately can have any doubt but that the centrosomes are the centres of that energy which produces cell division. Yet in the cells of the higher plants there are no centrosomes! The radiations appear and form the spindle. The process of division is precisely the same as in other cells but at the centres of the radiations are apparently structureless spaces. Within these structureless spaces must be the centres of energy. We may, I think, be sure that the chromosomes or collections of chromatin, the nucleus when it exists and the chondriosomes must be surrounded by a membrane which is impermeable to certain substances and it is probable that this membrane is composed of linin; but what the most primitive state of these structures may be we do not at present know.

What have we left which is absolutely necessary in the constitution of living matter? A complex substance composed of carbon, oxygen, nitrogen, hydrogen, phosphorus and sulphur, in which there must be centres of certain kinds of

activity. These centres may not be visible under any circumstances. Prof. Armstrong, in a previous number of *SCIENCE PROGRESS*, described from the chemist's point of view how conditions under which matter of the nature of protoplasm might have arisen. He spoke of "nuclei" arising but he uses the term, I understand, in the way I use "centres of activity" here. At any rate, I am afraid that biologists are likely to be misled by a term which means to them something so very different from what I understand is intended.

Knowing very little about enzymes, I am inclined to throw the next step in our advance in the knowledge of the origin of life upon them. Many if not all the phenomena connected with life appear to be dependent upon their presence. It is for the chemists to tell us of enzymes, which must certainly be intimately connected with those centres of activity which make the difference between living and dead protoplasm.

THE MYSTERY OF RADIOACTIVITY¹

A DRAMATIC critic ends his notice of a recent play with the words, "Radium, what crimes are committed in thy name!" We are scarcely so far advanced as to commit what are recognised as crimes in the name of the new "element" but not a few are engaged in gulling an ever-gullible public into the belief that it has magic virtues which make it a cure for all sorts of evils and in setting an entirely fictitious value upon it—to serve commercial ends. In thus acting, the medicine-men of to-day are but putting new wine into the old bottles which they have inherited from their very remote ancestors: some must know full well that there is nothing to justify the faith they preach, though others doubtless are the dupes of their own credulity and are fallen victims to the desire to believe in the occult which appears to be innate in us.²

The book under notice is one to be consulted by all who desire

¹ *The Interpretation of Radium.* By Frederick Soddy, M.A., F.R.S. Third edition. [Pp. xvi + 284, with illustrations.] (London: John Murray, 1912. Price 6s. net.)

² The use made of Radium is in no small measure a justification of Samuel Butler's criticism: "If people like being deceived—and this can hardly be doubted—there can rarely have been a time during which they can have had more of the wish than now—the literary, scientific and religious worlds vie with one another in trying to gratify the public!"

The effect of firing a profusion of bullets at a deal board would have is well known. It would seem that this is the kind of effect produced by the various "rays" emitted by Radium and that there is not the slightest reason to believe that it acts in any specific manner, as a chemical agent would: it but destroys living tissues, in the same way that X-rays, the rays from an electric arc lamp and strong sunlight destroy them. It has been used with some measure of success, in place of the surgeon's knife, to remove the surface form of cancerous growth known as rodent ulcer; but expert opinion favours the knife as far more certain, as it is difficult to be sure that the whole of the cancerous tissue has been got rid of when radium is used. It is more than difficult to believe that it can be effective in the case of deep-seated growths. That the infinitesimal proportion of Radium present in natural waters should have any useful effect is eminently improbable: those who encourage the belief in its efficacy certainly have no evidence to rely on beyond that furnished by their imagination. In most cases of disease, the factors leading to cure may be so numerous that it is impossible to single out one as the effective cause.

to understand what has been learnt of Radium and in what respects its behaviour is remarkable. The story is more than fascinating and it is told with remarkable lucidity, often rising to eloquence, by Prof. Soddy—who is one of the most noted workers on the subject of Radioactivity, the new branch of chemistry and physics brought into existence through the discovery of Radium. The present-day interpretation of Radium that it is an element undergoing spontaneous disintegration, was put forward in a series of joint communications to the *Philosophical Magazine* of 1902 and 1903 by Professors Rutherford and Soddy; moreover, if report speak truly, Prof. Soddy was the first to discover the production of Helium from Radium. In reading the book, therefore, we are drawing inspiration from the fountain head—and the stream is one which runs with quite exceptional clearness and fulness.

The book consisted originally of the matter of six public lectures delivered at Glasgow early in 1908; the present third edition is much enlarged and brings the subject of Radioactivity up to the middle of last year. It should be in the hands of every student of physical science—and in every school library: no person of intelligence should be able to read it without having his imagination fired and a desire awakened in him to know more of the wonders of science. The argument is developed so gradually and so clearly that few will have difficulty in understanding it.

As Prof. Soddy says, in discovering Radioactivity "science has broken essentially new ground and has delved one distinct step further down into the foundations of knowledge." But he goes too far in making the statement that it is a new primary science owing allegiance neither to physics nor chemistry as these sciences were understood before its advent, because it is concerned with a knowledge of the elementary atoms themselves of a character so fundamental and intimate that the old laws of physics and chemistry, concerned almost wholly with external relationships, do not suffice.

The fact is, Prof. Soddy is pardonably carried away by his enthusiasm and there are a number of over-statements, if not inaccuracies, in his earlier chapters which he will do well to modify in his next edition. Thus the one outstanding feature in connexion with Radium and the property of Radioactivity which it exhibits to an extraordinary degree, we are told (p. 24), is that

"The radioactive substances evolve a perennial supply of energy from year to year without stimulus and without exhaustion."

This is simply not true, as is fully shown later in the book—why then start with so misleading a statement? What too is a perennial supply? Gardening is so much in vogue in these days that most people know what perennials are—plants which the dealers say will live several years but which as often die during the first. This doubtless is not the connotation Prof. Soddy would select; the accepted meaning, perpetual, is incorrect. The word is again misused in Chap. III. A similar confusing statement on p. 32 might also be modified with advantage: it is undesirable in a scientific work to sacrifice accuracy to rhetoric, rather is it necessary to follow the rigid Euclidian method of argument throughout.

It is evident that in 1908 Prof. Soddy was irritated by the criticisms which were passed when the full meaning of the new discoveries was not yet apparent and the evidence could not easily be appreciated—otherwise he would not have written (p. 5): "Natural conservatism and dislike of innovation appear in the ranks of science more strongly than most people are aware. Indeed science is no exception." Either this statement should disappear from the next edition of the book or the position should be correctly defined. The assertion that there is dislike of innovation in the ranks of science is unjustifiable: we are ever on the look-out for new things and prepared to welcome the addition of an ascertained truth to the existing body of knowledge; the complaint commonly made of a fresh number of a journal is that there is nothing new in it of interest. And if conservatism be natural, as they are human beings, scientific workers, like most other people, are by nature *necessarily* conservative. If men generally were not conservative, society would have little stability. It is the first duty, moreover, of the scientific worker to be critical and to deny belief until satisfactory proof be given that he is justified in believing. It is just because so few are critical and logical that there are so few, even in the ranks of science, who deserve to be termed scientific—it is for this reason also that science is making so little progress among the people at large and that we can scarcely hope that it ever will make much progress. In the ranks of Science, as in those of an army, the majority are privates disciplined to do this or that work and to accept instructions; only the few are fit to exercise

independent judgment. The fact that there is so little criticism has also much to do with the slowness with which the knowledge so hardly won by generations of workers is being codified and properly utilised in developing a scientific conspectus.

In Radium a substance has been discovered which decomposes, apparently without rhyme or reason, at a perfectly constant rate and in so doing gives out an amount of energy altogether extraordinary in comparison with that given out in any of the cases of chemical change known previously—hundreds of times as much as can be derived from the combustion of an equal weight of coal. And the process is a very slow one in some of its stages, though very rapid in others; judging from the rate at which change is observed to take place, about 2,500 years may be expected to elapse before any given quantity is entirely dissipated.

If it be desired to form a picture of what is going on, we may imagine a vast heap of similar live shell—shell charged with an explosive—and that, in a given interval of time, a certain proportion of these explode spontaneously but without affecting the remainder; moreover, that in each subsequent similar interval of time always the same proportion of the remainder explode: obviously a smaller number will be destroyed at each successive explosion. Such is the behaviour of Radium. But to make the analogy complete, the shell must be thought of as packed with shot together with smaller shell; when these smaller shell escape, they in turn explode and disperse both shot and shell. But the rates at which the various smaller shell break down are different from that at which the parent Radium shell explodes. And the radium shell, it is supposed, are derived from still more complex shell—from Uranium, which breaks down so gradually that its complete conversion is estimated to occupy eight thousand million years.

When Radium was discovered, it was entered among the chemical elements in the metallic class, because it behaved like a metal in forming salts. When the further discovery was made that its radioactivity was consequent on its resolution into other substances, a dream was fulfilled which Mendeléeff had caused not a few chemists to dream by introducing the celebrated Periodic system of classification—a system which meant, if it meant anything at all, that the

substances regarded as elements—whether metallic or non-metallic—because they could not be resolved by any of the means at the chemist's disposal, were interrelated in such a way that there must be some genetic connexion between them. Now that it has been shown that three accepted elements of high atomic weight, Uranium, Thorium and Radium, are not simple substances, the probability that the elements generally are composite in their nature becomes very great indeed.

Prof. Soddy would retain the term element even for Radium. The question, "How can an element or the atom of an element change?" has given rise, he says, to many arguments of etymological rather than scientific importance. But science is only compatible with correct etymology—it is the duty of science to be correct in word as in deed. Prof. Soddy attempts to wriggle out of the difficulty in an interesting manner by arranging that :

"You may, if you like, regard the Radium atom as a compound of the atom of emanation and of the Helium atom which result on its disintegration, as it certainly is such a compound but you must make it quite clear that you do not mean a mere *chemical* compound, which may at will be formed from and decomposed into its constituents."

At the risk of being ranked as "a more or less random critic of younger workers in radioactivity," seeing no reason why even the younger worker should be spared from criticism, I venture to urge that the argument is illogical.

There can be no question that, owing to the discoveries under notice, the word element has now lost its significance in chemistry and that the difficulty of defining it is considerable. We cannot base distinctions on degrees of stability, as Prof. Soddy suggests should be done. But it is not easy to find a substitute. Perhaps, in the future, we may come to speak of *chemical primaries*, metallic or non-metallic. At one time, the term atom meant the unit quantity of *any substance*, simple or compound; it was customary to speak of the atom of water, for example. But when physical conceptions became paramount and Avogadro's theorem was accepted by chemists as their guiding principle, it became customary to apply the term molecule only to the kinetic or acting unit and to reserve the term atom for the ultimate elementary unit. Physicists, strangely enough, have never followed chemists in thus giving a precise

meaning to the terms molecule and atom; it would be a retrograde step if chemists were to resume the old practice, especially as two additional terms have been brought into use: that of *radicle*, applicable both to a single atom and to a group of atoms capable of acting as a whole; and that of *ion*, to signify the radicle which is active in electrolysis. Moreover, in nearly every case in which Prof. Soddy uses the term atom, the correct term to use is molecule. The atom, it is true, will become more than ever an ideal, if such reservation be made; but it is an ideal we need.

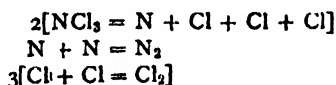
The properties of the radioactive elements are most surprising in many ways. There have been chemists who have expected doubtless that some day sufficiently powerful means would be discovered enabling us to decompose elements; no one had dreamt, however, of elements undergoing decomposition spontaneously and thereby themselves affording the long-expected proof of their composite nature; and no one probably had ever thought of the possibility of such a vast amount of energy being stored up in a substance as is now known to be stored up in Radium.

How are we to explain the change which it undergoes—is it altogether without analogy, we may ask—is Prof. Soddy justified in asserting that the old laws of chemistry and physics do not suffice? Of late years, it has been the favourite doctrine of those who dub themselves physical chemists that a great variety of chemical changes are taking place unperceived at very slow rates and that when such changes are caused to take place rapidly by the intervention of a catalyst this but serves to hasten the rate of change. The spontaneous decomposition of the radioactive substances is not surprising from this point of view.

Nor is it surprising that the change should take place at a constant rate—the behaviour of Radium, in fact, is simply that characteristic of every changing substance: as chemical change always takes place at some constant rate depending upon the conditions. What is remarkable is that we are unable to influence the rate of change—either to retard it or to hasten it by any of the means which are ordinarily effective. Furthermore the amount of energy dissipated is phenomenally large. Wherein lies the explanation of these peculiarities?

The first stage in the decomposition of Radium involves the formation of the so-called emanation and of Helium—which are

two absolutely neutral substances apparently. It cannot be a compound of such substances and yet they are obtained from it: either or both must be present in it in some active form. Many parallel cases are known to us. When nitrogen chloride is exploded, it gives rise to nitrogen and chlorine gases, neither of which can conceivably be present as such in the chloride: in point of fact, there is every reason to believe that the molecule of the chloride is resolved into its constituent "atoms" and that these then unite in new ways to form molecules: it is in this last operation that the energy is liberated. Thus



It is only necessary to suppose that the molecule of Helium as we know it, like the molecule of nitrogen as we know it, is composed of several "atoms" of—let us call it—*protohelium* and that the atoms of protohelium have *intense* affinity for one another—an affinity so intense that it is far beyond anything we have experienced in the case of any other element.

When argon was first described in 1895 by Rayleigh and Ramsay, I ventured to assert such a view in explanation of its apparently complete inactivity. What is true of argon, is true doubtless of all its companions in air—helium, neon and krypton.

In the light of my hypothesis, chemical primaries such as Uranium, Thorium and Radium are comparable with the complex hydrocarbons of the paraffin series represented generally by the formula $\text{C}_n\text{H}_{2n+2}$. When the paraffins are heated, they are decomposed in a variety of ways: one way is that, time after time, the elements of a molecule of hydrogen are removed, a hydrocarbon being produced containing proportionately less hydrogen; thus



Such changes correspond to those which the radioactive primaries undergo in losing the elements of a molecule of helium time after time. But some of the immediate products in the case of the hydrocarbons are unstable and at once undergo change into an isomeric substance; this is a weightless change

and corresponds, it may be supposed, to that which happens when terms in the radioactive series are formed without any apparent change in the weight of the molecule—changes in which only β and γ rays are given out.

Finally, we have to consider the rates at which Radium and other radioactive materials undergo change—why the rate is constant in each particular case. Why, as Radium decomposes so slowly, does it decompose at all; why does it not all blow up suddenly, like an ordinary explosive? There is but one explanation—that, like the other *mere chemical compounds* Prof. Soddy speaks of so slightly, it is always being decomposed reversibly—into protohelium and something else, the which products reunite more frequently than they part company and escape, the protohelium after it has united with itself; the Radium does not blow up, because of the intense affinity of protohelium for its companion product of change; for a similar reason, heat is without influence on the rate of change and there is no helium to be seen in the spectrum of Radium.

It would be surprising that Prof. Soddy and other workers have so long overlooked the potentialities of protohelium, were it not human nature to have chief affection for one's own children: to be blind to their faults and disinclined to seek virtues in those of others. I venture, however, to suggest that it were time to discard the fiction that the gases of the argon family are monatomic molecules which has so long retarded progress.

Protohelium apparently is the wondrous material at the root of radioactivity.

The terms of short life in the radioactive series are to be regarded as compounds in which the affinity of the constituent radicles for each other is slight. Radium or uranium even and the most ephemeral of the radioactive products which it furnishes may be contrasted the one with say sodium chloride or carbonate, the other with nitrogen chloride or ammonium carbonate: they are separated by a wider energy interval but only in degree.

What has been said, it may be hoped, will in no way diminish the attractiveness of Prof. Soddy's tale of wondrous scientific achievement.

H. E. A.

REVIEWS

The Origin of Life: Being an account of Experiments with certain superheated Saline Solutions in Hermetically Sealed Vessels. By H. CHARLTON BASTIAN, M.D., F.R.S. Second Edition. [Pp. 98 with 12 plates.] (London: Watts & Co., 1913. Price 3s. 6d. net.)

DR. CHARLTON BASTIAN is nothing if not persistent. The volume under notice is a second edition of his well-known essay, together with an appendix—termed important on the title-page, this being a paper read by him, so recently as November 19, 1912, before the Pathology section of the Royal Society of Medicine. Apparently he has been spurred to this fresh effort by the discussion on the Origin of Life which took place at the British Association in September last.

Dr. Bastian's essay is a pathetic document, as showing how easy it is for a man to persuade himself into believing in the impossible. Not a few scientific workers will be in full sympathy with him on account of the transparent sincerity of his convictions, though they may refuse altogether to accept his experiments as satisfactory. The essay contains his well-known indictment of the Royal Society, who have declined to publish his papers. But he is wrong in regarding himself as injured—the course he has taken in consequence of the refusal meted out to him by "our premier scientific Society" has not only brought his fancied wrongs prominently under notice but has secured far greater prominence for his views than they would have had if they had been officially recorded. The Royal Society has two kinds of archives—those which technically rank as such and its official publications: it is well known that these latter are the highest form of decent burial the scientific worker can achieve. They are to be found resting peacefully on the shelves of the fellows and of public libraries but the evidence is conclusive that they are rarely consulted here and that they are practically unknown abroad. As self-erected monuments of industry and scientific precision, many of the memoirs the volumes contain are magnificent—but they rarely enter into practical politics. Had the Royal Society desired to nip Dr. Bastian's heresies in the bud, they would probably have ordered the publication of his communications in their Transactions. In fact, Dr. Bastian has failed to realise that Huxley and Michael Foster his follower were wags both and that their real object must have been to give prominence to his views.

At present the Royal Society is suffering under the load of its traditions and its superlative respectability but its inanition is deplorable. Some day it may appreciate the sacred nature of the trust committed to it and once more become a factor in the progress of science. Even papers such as Dr. Bastian's will be accepted and read, fully and critically discussed and—if not withdrawn by consent or request of the author—published together with the discussion, so that all who run may read. The Society will then rank high by reason of the sympathy which it will extend to all serious workers and its best safeguard will be the reputation it will enjoy as a centre of unsparing but honest criticism; in that far-off time maybe science will have its golden days and will be honoured as the protector of the public at large against false belief and pretence.

There is much food for thought in Dr. Bastian's volume. It would be wrong to say seriously that it is full of absurdities—and yet such an expression is almost the only one that does justice to its argument. The immensity of the problem considered is patent: Dr. Bastian's failure to appreciate the gravity of the issues his contributions raise is only too obvious. Psychologically his attitude is one that deserves most careful consideration—it illustrates both the difficulty that attends the interpretation of the complexities of nature and our human tendency to take ourselves seriously as capable exponents of her workings. Dr. Bastian claims to have produced *Torula* in his latest experiments from solutions containing only, to each ounce of distilled water, either a few drops of a dilute solution of sodium silicate together with about three times as many drops of *liquor ferri pernitralis* or a few drops each of a dilute solution of sodium silicate and dilute phosphoric acid together with a few grains of ammonia phosphate. In his latest experiments, he used pure colloidal silica prepared by Graham's method in place of the silicate.

In opposing Dr. Bastian, Huxley doubtless was influenced mainly by his feelings but sustained argument may now be substituted for his sledge-hammerism. In the interval, we have learned much regarding the structure of the constituents of the protoplasmic complex—the nature and functions of enzymes have been made more or less clear to us—even simple organisms such as Dr. Bastian asks us to believe were produced *de novo* in his tubes have been shown to be of extraordinarily complex structure and capable of exercising both the synthetic and analytic operations characteristic of organisms far higher in the scale—the chemist has also discovered that Nature has developed extraordinary powers of selecting out a certain limited set of materials for use in her building operations: those who understand these things feel that it is simply inconceivable that life can ever arise from materials such as Dr. Bastian has used and during times such as were covered by his experiments. Bacteriologists have accumulated a vast fund of experience: if the calling of living things into being were the easy process he imagines, his observations would have been corroborated and his contentions admitted over and over again.

With regret we must conclude that Dr. Bastian has never been a competent critic of his own proceedings but he is in no way singular. Much of the so-called research work of our time would never see daylight if those who perpetrate it were better informed and sufficiently modest to be conscious of their inability to deal with the tasks which they have had the temerity to undertake. This is the coming difficulty in science; the rank and file will continue to do good hoe and spade work so long as they are prepared to subordinate themselves to competent leaders but it will be possible to trust but the very few to deal with the more comprehensive problems or to base generalisations upon the scattered observations of the multitude. It is in this direction, we may hope, a regenerate and virile Royal Society will be able to serve the State—in promoting Natural Knowledge by judiciously organising, criticising and controlling the exercise of scientific effort.

The Growth of Groups in the Animal Kingdom. By R. E. LLOYD, M.B., D.Sc. (Longmans, Green & Co.)

UNDER an unassuming title this book conceals a most ambitious aim, no less than an attempt to solve one of the root-problems of zoology, for the term "group," as defined by the author, is used to include everything from a sport represented by two or three specimens to a new species and the question discussed under the

caption of the "growth of groups" is nothing less than the origin of species. We may say at once that, though we do not think that the author has succeeded in his aim, he has certainly collected together some most interesting facts. The discovery that plague was communicated to man by the rat-flea led the Government of India to take measures to collect and destroy rats on a much larger scale than anything of this kind that had been previously attempted; it led further to an investigation of the number and distribution of the varieties of rats found in India. It was the privilege of the author to assist in these investigations and from them were derived the ideas which are embodied in this book.

What Mr. Lloyd has been able to show is briefly this: (1) that *Mus rattus*, the so-called "old English" black rat, is the dominant species over the greater part of India; (2) that this species exhibits marked colour varieties and that the most frequent colour variety is not black but greyish brown, very similar in colour, in fact, to the "common" rat of England, *Mus norvegicus*, from which it, like all varieties of *Mus rattus*, is separated by a number of anatomical marks; (3) that these colour-varieties are sometimes confined to definite districts, such as mountainous regions but sometimes occur in colonies in the heart of a population consisting of the "normal" variety; (4) that these colonies may be of any size, from a "group" consisting of two or three individuals to assemblages of much larger size which may include hundreds of individuals.

He shows further that there is evidence that practically the same variation must have originated independently in widely different centres and that there is some evidences that individuals of the same colour-variety have a tendency to consort and mate together.

Mr. Lloyd is an ardent believer in the mutation theory of De Vries and, of course, he sees in the distribution of these colour-varieties evidence in support of that theory. According to him there can be no doubt at all that these colour "mutants" have been born (through unknown causes) of "normal" parents and have then proceeded to generate offspring like themselves, which have constituted the group. It is in this way he imagines that new varieties and ultimately new species have come into being. Mr. Lloyd has performed no breeding experiments with his mutants and all his evidence consequently is indirect. Now all who know the state of research into problems of heredity at the present day are aware that nothing would give the orthodox Mendelian greater pleasure than to assist at the birth of a new mutation and that the claim of De Vries to have done so is gravely questioned by many of the most trustworthy workers in this field. Mr. Lloyd seems to be totally unaware that the celebrated *Oenothera Lamarckiana* labours under the suspicion of being a hybrid itself and that it is quite possible that the various mutants to which it has given rise may be due to nothing but the segregation of the different factors which have entered into its complicated ancestry. If in the normal population of *Mus rattus* there exist several strains of heredity which, for all we know, may have existed since *Mus rattus* was a species at all; further, if some of these strains are dominant over others, then there will be always a sporadic appearance of apparent new varieties due to special concatenation of circumstances which favour the appearance of recessive strains in certain localities. On the fundamental question of the origin of a new mutation his observations throw no new light.

We mentioned at the outset that we do not think that Mr. Lloyd had solved the problem of the origin of species. Mr. Lloyd has some caustic comments to make on the different conceptions of species held in practice by different types of naturalist. He is certain that if some of the colour-varieties which he encountered.

had been sent home to museum specialists they would have been registered as new species. Possibly he is right and yet too much blame must not be given to those unfortunate but indispensable specialists. Possibly, like most conservative naturalists, they have the conception of a species as a *group of animals occupying a definite area, bound together by many constant characters and freely interbreeding, whose constitution is adapted to the environmental circumstances of that area*. They would be the first to recognise that most of their specific determinations, especially when they deal with collections of animals from unexplored areas, are and must be provisional and would be ready to modify them as soon as fresh evidence on the subject was available.

The real enigma in the origin of species is not the origin of slightly different strains within the same stock but the *origin of adaptations*. On this subject, as on what he conceives to be "Darwin's theory of selection," Mr. Lloyd has some extraordinary remarks to make. It is, he thinks, of the essence of Darwin's theory that variation should be small and should be chaotic, *i.e.* in all directions. Further, he thinks that natural selection is an attempt to explain the unknowable, *i.e.* adaptation.

It may surprise him to learn that Darwin was just as well acquainted with the existence of "mutants" as De Vries and if he did not think that they had been of importance in the formation of new species, it was not on account of any philosophical objections to such an assumption but on account of many weighty practical considerations which are set forth in detail in his works. As to variations being "chaotic," Darwin, who spent a life-time in collecting all the information he could about variation, was in a better position to judge than Mr. Lloyd. He found that there was no part of an animal or plant which could not be made to vary in any direction which man desired, as was evidenced by the whimsical peculiarities of "fancy" strains of domestic animals and plants: and it was a fair inference that if man could always find the variations he wanted, they must occur sufficiently frequently to allow natural selection to modify a species in any direction.

What hazy metaphysical notions Mr. Lloyd has in his head to permit of his calling adaptation "unknowable," it is hard to guess. Adaptation is part of the present order of nature, just as is the distribution of land and water and we have reasons for believing that neither in its present form has existed from all eternity; and it is the function of science to explain the present from the past. Mr. Lloyd's philosophical reflections are clothed in what he imagines to be an epigrammatic style but we cannot think that such aphorisms as "Dissent is the outcome of a difference of judgment which is inherent in the dissenter" add anything to the forcefulness of his arguments.

In conclusion we cannot give Mr. Lloyd better advice than to engage in a renewed and serious study of Darwin's works—more especially that entitled *The Variation of Animals and Plants under Domestication*—and to weigh carefully the concluding passages of that monumental work before putting forward new ideas on the origin of species.

E. W. MACBRIDE.

Sylviculture in the Tropics. By A. F. BROWN. [Pp. 309, figs. 96. 8vo.] (London: Macmillan, 1912.)

THIS refreshing work differs materially from other modern text-books of forestry issued in this country. Of the latter, all the larger ones sufficiently accurate to be worthy of consideration owe their publication—as does the book under review—to

former officers of the Indian Forest Service, yet they are in the main adaptations or actual translations of German works and deal specially with European forestry. Mr. Brown's book deals with tropical silviculture and contains much information collected by the author during his wide experience in the forests of India, Ceylon and the Sudan. Mr. Brown evidently recognises that in dealing with problems outside the mere routine work of continental forestry it is essential to have a special knowledge of trees and of the conditions under which they feed and grow; accordingly he makes use of his well-known acquaintance with systematic botany and his evident study of plant ecology in the opening chapters, which are devoted to the discussion of the factors influencing the existence of forests. Much of this information will also be of interest to botanists, for we find here interesting facts concerning trees occupying soils differing in constitution or moistness and in sites differing in climate or altitude or exposure. The examples described are largely taken from forests in the tropics of the Old World. Very varied are the matters discussed; for instance, the significance of depth of root in relation to resistance to drought is exemplified by reference to Mr. R. S. Pearson's account of the damage done to forests in the Madras Presidency during the drought of 1899-1900. Among the suggestive facts that came within the author's range of observation may be cited the remarkable germination of a viviparous Dipterocarp (*Vatica*) that grows in annually inundated sites in Ceylon. In the chapter dealing with the living environment of trees the author gives interesting particulars and illustrative examples of matters varying from the succession of vegetation in forest clearings to the kinds of seeds distributed by deer and elephants, the pollination of parasitic Loranthaceæ by birds, the damage done to forests by plagues of rats and the intense dislike of elephants of white objects such as whitened posts or white-barked trees, which are therefore wantonly destroyed by these animals. In the chapter on "Man and Domestic Animals" Mr. Brown falls into the assumption, which is by no means justified, that "in olden times . . . the greater part of the globe was forest clad." An analysis of the climates of cold or dry deserts and various grasslands renders it probable that "in olden times" there were always immense areas not clad with forest. Yet the forester readily comes to Mr. Brown's impression because he may have an exaggerated idea of the climatic change induced by disforestation and in Europe he knows not only of wholesale destruction of forest by man and its degradation to heath or peat-bog but also of evidence of the prehistoric existence of vast forests where bogs now prevail; within the tropics and subtropics the forester also sees the change of dense forest into open savannah, grassland or waste area, induced by fire (perchance not due to man's agency) possibly combined with the subsequent activity of grazing or browsing animals. Instructive examples of such changes are described by Mr. Brown and accounts are given of the forest-destroying powers of the goat and of the even more destructive camel. To these two kinds of animals has been attributed the disappearance of woodlands supposed to have existed formerly in the wadis of Upper Egypt where desert now reigns. In connexion with the discussion on the effect of fires the information is given that protection against fire has caused some teak forests in Upper Burma to change into evergreen forest, in which teak can no longer reproduce itself. Among the important factors influencing forests is light and it seems a pity that Mr. Brown should not have given in his book some discussion of its significance, since so much botanical work has been published recently in regard to the amount of light required by various species of plants including trees and the practice of forestry is so largely a matter of the proper regulation of light.

The remaining sections of the book deal with practical operations of the forester. The information is conveyed in a clear and interesting manner and includes adequate recognition of necessary deviations from ordinary European practice, for instance in connexion with the water supply in nurseries and the making of coppice.

A number of instructive photographs and other illustrations add to the value of this work, which may be recommended not only to foresters but to all engaged in the cultivation of trees and shrubs within the tropics.

PERCY GROOM.

British Violets: A Monograph. By Mrs. E. S. GREGORY. With an introduction by E. Claridge Druce, M.A., F.L.S. [Pp. xxiii + 108, 32 illustrations.] (Cambridge: W. Heffer & Sons. Ltd., 1912.)

MRS. GREGORY, whose work on the Eu-Violas is well known, has given us a useful book on this sub-genus. The proof of the value of such a work is in the using. The reviewer has worked through the long series of violets in his own herbarium and has found that, in most cases, the descriptions, notes and figures leave but little doubt as to the identification. The notes on distinctive features, when given, are very helpful and it is rather to be regretted that in some cases they are not more complete. For instance, it is doubtful whether var. *pseudomirabilis* of *V. Riviniana*, Reichb., could be determined with any certainty from the account given.

There is, perhaps, a tendency to rely too implicitly on the opinion of continental botanists who have examined British specimens. In the preliminary stages of the study of such a "critical" set of plants as these, it is generally necessary to consult foreign experts but observation of plants in the field may overrule the judgment of an absent authority—who knew nothing at first hand of the habitat of the specimens under examination. Varieties of different species often approximate in form, although the species are quite distinct. Hence knowledge of the range of forms in any district may suggest the probability of specific identity of very unlike plants—an identity at which the referee, seeing only a few plants, could never guess. Indeed, a study of the range of form possible for each clearly distinct species is urgently needed in all "critical" groups.

No fewer than seventeen supposed hybrids are mentioned. Some hesitation is perhaps natural in accepting all of these. Indeed the authoress herself expresses doubt in some cases. The whole subject of hybrids in the British Flora is worthy of careful study. Combination of the characters of two well-marked species, especially if accompanied by sterility, may be good evidence and probably, in general and under certain conditions, it is so but direct confirmatory evidence from actual crossing is desirable. In particular, doubt may well be felt in approaching such a name as *V. canina* × *V. lactea* × *V. Riviniana* (p. 96).

These remarks, however, are in no way intended to detract from the commendation of the book. Mrs. Gregory did not set out to settle all the doubtful points respecting the Violets. This would have required more than the twenty-five years of study which she has devoted to the group. She intended to give a clear working account of the violets occurring in this country and in this she has very largely succeeded.

A special word of praise must be given to Miss Mills's drawings; these are numerous and very well executed. The photographs of herbarium specimens, too, are clear and helpful.

Monographs on Biochemistry. *The Simple Carbohydrates and the Glucosides.*

By E. FRANKLAND ARMSTRONG, D.Sc., Ph.D. Second Edition. [Pp. 171.]
(London: Longmans, Green & Co., 1912. Price 5s. net.)

ONE of the most gratifying signs of the success which has attended the series of Monographs on Biochemistry is the fact that new editions of the earlier issues are now appearing before the original programme of publication has been completed. The scheme outlined by the editors, in the general introduction to the series, is thus being faithfully adhered to and it is obvious that it is being appreciated.

In this second edition of his book, Dr. E. F. Armstrong has expanded and modified the original work in a number of ways, the result being that the present volume appeals forcibly both to the chemist and biologist. The task of selecting the fundamental points of the chemistry of the sugar group from the voluminous literature of this branch of research is in itself no easy one. To present the facts in a natural and logical order, whilst keeping the theoretical aspects of the subject in the foreground, is still more difficult and a careful review of the present work justifies the opinion that Dr. Armstrong is to be congratulated warmly on having produced a memoir of permanent value.

The opening chapter gives a clue to the spirit in which the book is written. Dr. Armstrong is obviously of the opinion that the chemistry of the sugar group can only be properly approached from the constitutional standpoint and in this he is right. Throughout the book, the structure of the sugars and their related compounds is dealt with in an exceedingly lucid manner and thus the work is free from the reproach of being an empirical tabulation of compounds and their properties.

Compared with the first edition, considerably more space has been devoted to the phenomena of mutarotation and isomeric change; the growing importance of the biochemical relationships of the sugar group has also received due recognition and a number of the rarer compounds have been fully described. The bibliography has been thoroughly revised and brought up to date.

The work should be valuable both to the organic chemist who is interested in biochemical problems and to the biologist who desires to gain an insight into the somewhat complex chemistry of the simple carbohydrates. The first edition of the book has also, in the reviewer's experience, stood the test of being used as a special text-book by students preparing for research work on sugars and the present issue cannot fail to be more useful still in this respect.

There is one point in which future editions of this and other members of the series might possibly be improved. That a highly specialised technique is required for work in the sugar group is well known and investigators new to this work may well be discouraged by the practical difficulties encountered. The suggestive manner in which Dr. Armstrong's book is written is likely to attract new workers to this field and this desirable result would be greatly promoted if, in future editions, he could find it possible to add a series of practical notes, derived from his own experience, on the manipulation and purification of the sugar series.

J. C. I.

Electromagnetic Radiation and the Mechanical Reactions arising from it.

By G. A. SCHOTT, B.A., D.Sc., Professor of Applied Mathematics at Aberystwyth. [Pp. xii + 330 and 7 appendices; 51 figs.] (Cambridge University Press. Price 18s. net.)

THE subject proposed for the Adams Prize Essay in 1908 was "The Radiation

from Electric Systems or Ions in Accelerated Motion and the Mechanical Reactions on their Motion which arise from it." The book under review is an extension of the Prize Essay, most of the additional matter being introduced in seven appendices occupying the last 125 pages. As might be anticipated from the title, the essay is deductive and mathematical rather than constructive and physical. The object of the author has been to establish mathematically the foundation for any theory of matter based on the electron, whether it be the electron of Lorentz or that of Bucherer or that of Abraham.

After a brief discussion of the fundamental equations of the Maxwell-Hertz-Lorentz electron theory, Chapters II. and III. are devoted to the subject of retarded potentials and the point potentials of Liénard and Wiechert. These point potentials are discussed in Chapters IV., V. and VI. and applied to the determination of the electromagnetic field in various special cases of the motion of a point charge. In Chapter VII. the motion is assumed to have a single period, whilst in the following chapter more complicated periodic motions are considered and in Chapter IX. non-periodic motions are dealt with. In Chapter VIII. there is also a discussion of the precessional motion of a vibrating system, which is of importance in the theory of the Zeeman effect. Chapter X. is devoted to the field near the orbit of the vibrating charge, the next to the consideration of the equations of motion of the moving charge itself and these are extended in Chapter XII. to a group of electrons. The author states that the appendices, with the exception of the first, are mainly devoted to remedying the defects in Chapters XI. and XII., which, owing to the shortness of time allowed for the essay, were not treated at all adequately. The first appendix deals with the Doppler effect.

Not the least pleasing feature of the book is the number of clear diagrams by means of which the author illustrates and explains the mathematical processes and results.

Vector analysis is used throughout and the distinguishing type and symbols adopted are excellent.

The book is one which can be confidently recommended to all who know something of electromagnetic theory and the methods of vector analysis and wish to understand the recent developments of the electron theory.

Electric Lighting—and Miscellaneous Applications of Electricity. A Text-book for Technical Schools and Colleges. By WILLIAM SUDDARDS FRANKLIN. (New York: The Macmillan Company, 1912.) [Pp. 239 ix. chaps., with 197 figures.

THE title of this book is misleading, as less than 100 pages deal with electric lighting. The contents are best described by the word "miscellaneous" in the sub-title. The chapter contents are as follows: 1, Costs and methods of charging; 2 and 3, wiring and transmission lines; 4 to 7, photometry, lamps, illumination; 8, electrolysis and batteries; 9, telegraphs and telephones; Appendix A, dielectric stresses; Appendix B, problems. Much of the material is good but the arrangement is peculiar and Appendix A gives one the impression of having introduction by mistake. The practical data apply to American conditions and things almost unknown here are given as standard practice.

Apart from such typographical errors as "killowatt" and "magdetite," we note on p. 87 "two million ergs (or 0.2 of a watt) per second." The statement on p. 229 that bridge duplex is specially prevalent in England is hardly correct. Fig. 136 would not work with the batteries as arranged and Fig. 98 is ridiculous.

Fig. 82 is borrowed from Mrs. Ayrton without a word of acknowledgment. A very good point is the large number of references to papers and books on the various subjects. The book is exceptionally well bound.

Outlines of Evolutionary Biology. By ARTHUR DENDY, D.Sc., F.R.S.
[Pp. ix + 454.] (London: Constable & Co., Ltd., 1912. Price 12s. 6d. net.)

PROF. DENDY'S intention in writing this book evidently has been to purvey biology for the million and badly enough it is wanted: mais l'homme propose et le bon Dieu dispose. Will it serve the appointed purpose? It has been most favourably noticed by the Press but does this mean anything in these days of grace, now that reviewing is a lost art and very few of those who have an opinion dare express it? Owing to the fact that the specialist too often lacks sense of proportion and is apt to live a life apart and have no inkling of the depths of ignorance of those whom he addresses, his opinion on a work intended for popular consumption may be of less value than that of the ignoramus thirsting for information. It is therefore permissible that a book such as that under notice should be criticised from the point of view of those who have no special knowledge of its subject-matter and yet are most anxious to learn; indeed it would be much better if books were sometimes reviewed by those for whom they were written and not by those who presume to understand them: if only we had the opinions of schoolboys and schoolgirls on the works that are provided for their consumption, there would be some chance of a chastened race of authors being evolved who would write books worth reading, as if writers realised how often their productions are spoken of in very uncomplimentary terms by juvenile readers who are forced to use them, their self-sufficiency might be abated and they might eventually even be overcome by some sense of modesty and retire from the field: those who "feel a want" in the course of their educational ministrations would more often seek comfort in some less harmful form of exercise than that of attempting to write a book. When the new Socialism is established, no doubt such things will be provided against.

It is easy to agree with the opinions expressed by the author in the earlier part of his preface. There is no doubt that biology, the fundamental science of living things, is not properly encouraged by educational authorities in this country—but is the fault entirely theirs? Can the subject be taught satisfactorily in schools, is it sufficiently developed? That attention is usually directed to the more special branches no one will deny but is not this because of our more than relative ignorance of the general subject? Is it possible at present to write a book that will be *of use*—we desire to emphasise the "of use"—to those who have no special biological training as well as to students who have taken the ordinary first year's course and largely with a view to meet the requirements of those who wish to familiarise themselves with the rapidly accumulating results of biological investigation and the bearing of these results upon the problems of life?

It is only necessary to read through the opening chapter of Prof. Dendy's book to realise how great the difficulties are: the author, like most zoologists it is to be feared, obviously does not possess sufficient knowledge of chemistry and physics to discuss the subject dealt with in it—the nature of life: to us it seems that when the beginner has read through the chapter, he will know less than when he began: like the frog in the fable, he will be puffed out with importance, as he will be

equipped with sundry fine words and phrases but his mind—if he have one—will be in a hopeless muddle.

At the end of eleven pages of terribly thin talk—there is no other term for it—he will scarcely be comforted on hearing that “the ‘soul’ of Descartes’ philosophy corresponds more or less closely with the ‘vital force’ of some more recent writers and the ‘entelechy’ of others,” especially as he is left without an explanation of the blessed word entelechy.

To attempt to elucidate the nature of life in so brief a space is out of the question : the whole chapter should be scrapped whenever a new edition of the book is prepared. Until proper training has been given in things fundamental in chemistry and physics, it will be impossible for students to grasp even the simplest conceptions of vital problems and the present-day biologist is certainly incompetent to discuss the philosophy of so vast a subject as that of the nature of life, which is admittedly an infinitely intricate nexus of complex chemical events. If we are to write books that are to be *of use* to students, we must school ourselves to talk only of things we can and do comprehend and they can understand.

The later chapters of the book are open to similar criticism, in so far as they do not relate to matters specifically zoological, which are usually treated clearly and in an interesting manner.

To refer to only a few points—surely it is undesirable even to mention to absolute beginners the attempts that have been made to explain the dynamics of mitosis—of which we are in absolute ignorance from A to Z, whatever cytologists may say.

Variation and heredity, subjects of infinite importance, are dealt with in sixty-two pages in Part III., chapters xi-xiv. : the treatment is of the kind to be expected in articles of the popular magazine type ; no beginner could possibly make much of the jumble of statements brought under notice. In this section, the inheritance of acquired characters is dealt with in a way which makes it pretty clear that the author is a believer in the doctrine : parenthetically we may say that whatever the force of the arguments in its favour may be, we are convinced that if there be one character that is not acquired it is the art of writing books successfully—this seems to be born in the very few.

The kind of logic used in dealing with the subject of inheritance will be apparent from the following statement on the last page but one of the book :

“Man has indeed acquired a degree of control over his environment and over his own destiny which distinguishes him from any of the lower animals but at the same time the conditions of his life have become far more complex and the young, at any rate in civilised communities, have to go through a long course of education before they are fit to enter upon the struggle for existence on their own account. Amongst the lower animals, all or almost all the faculties necessary for existence are directly inherited from the parents, incorporated in the organism itself ; but man inherits in this way only a relatively small proportion of the powers which he requires to carry on his life. The greater part of human experience is of too recent origin to have become heritable ; it has to be acquired afresh by education in every generation and in this respect is strikingly contrasted with the instincts of the lower animals.”

Much of the difficulty in discussing this all-important subject arises probably from the loose manner in which the term “acquired character” is used at the present time. The untutored human being apparently is much like the exposed photographic plate—the latent image is there but requires to be developed ; it may

be developed to various degrees of intensity but no development can bring out a non-existent detail. A so-called "acquired character" may well be nothing more than a developed character and not in any true sense one that is acquired. It is as if a man found himself the possessor of various factories full of machines of which he has little understanding: he sets to work and learns gradually to make use of them; sooner or later he is able to use some of the machines efficiently, others he never makes use of, either because he cannot understand them or because they were imperfect when they came into his possession or because he is never called upon to set them in action, there being no demand for the articles which can be made with their aid. He is even able to turn out new machines like the old ones but is strictly limited to copying these, as the only templates at his disposal are those from which they were made: willy-nilly therefore he is forced to copy.

To be frank, we are of opinion that the author lacks not only the borderland knowledge but also the critical power that is needed in writing such a book—the power to take himself to task on every page and ask himself if he be not making a fool of himself in stating this or that: without this, no one, in these days, should attempt to write for babes and sucklings. Far too much is attempted and what is written is put together far too loosely.

If the book were deprived of the cheap attempts at "philosophy" which disfigure it and reduced to a common-sense account of the things which are really known to zoologists, it would doubtless be of value—as the technical descriptions are usually well written and well illustrated. As it stands, however, it is a most misleading work—the student who had swallowed it as gospel would only be a thing of shreds and patches, full of bombast and loose jargon but entirely lacking in true understanding of the subject.

The book convinces us, in short, that educational authorities will be right in giving but little encouragement to the teaching of general biology until it can be placed on a logical footing. Loose scrappy talk must at all costs be kept out of the schools.

It may be, however, that we are blaming the author for the faults of his class and that what we have to object to is the way in which the biologists of our time are prone to talk big of things of which they have no real understanding. By wrapping up an endless number of factors in terms such as environment—by speaking of stimuli without giving the least idea what a stimulus is and how it acts—it is easy to produce a great impression of learning. It is noteworthy that John Stirling, in a letter he wrote to the author on the appearance of *Savior Resartus*, took exception to various new words Carlyle used—among others "environment"; soul-satisfying as such epithets are, when analysed they amount to little: in the end we must admit that we cannot yet ask a single clear question, let alone answer one, about life.

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